PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Clinical Lectures on the Practice of Medicine. By ROBERT J. GRAVES, M. D., &c. Second Edition. Edited by J. MOORE NELIGAN, M. D., &c. Dublin: Fannin and Co. 1848. Vol. I., Chapters XXVII. and XXVIII.

Reports on Epidemic Cholera. Drawn up at the desire of the Cholera Committee of the Royal College of Physicians:— I. *Report on the Cause and Mode of Diffusion of Epidemic Cholera.* By WILLIAM BALY, M. D., F. R. S. London: Churchill. 1854. 8vo, pp. 345. II. *Report on the Morbid Anatomy, Pathology, and Treatment of Epidemic Cholera.* By WILLIAM W. GULL, M. D. London: Churchill. 1854. 8vo, pp. 220.

On the Mode of Communication of Cholera. By JOHN SNOW, M. D. London: Churchill. 1854. 8vo, pp. 162.

The Nature of Cholera Investigated; with a Supplemental Chapter on Treatment, Addressed to Junior Practitioners. By JOHN GEORGE FRENCH, F. R. C. S. London: Churchill. 1854. 8vo, pp. 152.

Asiatic Cholera; its Symptoms, Pathology, and Treatment. By RICHARD BARWELL, F. R. C. S. London: Churchill. 1853. 8vo, pp. 219.

A Pathological and Practical Treatise on Epidemic Cholera; its History, Causes, Various Forms, and Treatment. By O'B. MAHONY, L. F. P. & S., L. S. A. London: Churchill. 1853. 8vo, pp. 190.

Statistics of Cholera. By Assistant Surgeon EDWARD BALFOUR, of the Madras Army. Madras: Pharoah and Co. 1849. 8vo, pp. 70.

Appendix (C.) *to the Report of the General Board of Health on the Epidemic Cholera of* 1848 *and* 1849. Presented to both Houses of Parliament by Command of Her Majesty. London: 1852. 8vo, pp. 93.

Report of the Commissioners of Health, Ireland, on the Epidemics of 1846 *to* 1850. Presented to both Houses of Parliament by command of Her Majesty. Dublin: 1852. 8vo. pp. 79.

Report of Sir William Burnett *on the Cholera which attacked the Fleet in the Black Sea, in August,* 1854, *more particularly as relates to Her Majesty's Ships "Britannia," "Albion," and "Trafalgar."* Printed by Order of the Right Hon. the Lords Commissioners of the Admiralty, 1854. Folio, pp. 15.

A Letter to the President and Fellows of the Royal College of Physicians, in relation to the evidence cited in their late Report on the Treatment of Epidemic Cholera. By Joseph Ayre, M. D., &c. London: Churchill. 1854. 8vo, pp. 71.

Thoughts on Cholera. By Edwin Hearne, M. B., &c. London: Churchill. 1853. 8vo, pp. 56.

Result of an Inquiry into the invariable Existence of Premonitory Diarrhœa in Cholera, in a Series of Communications to the Registrar General. By David Mac Loughlin, M. D. London: Churchill. 1854. 8vo, pp. 61.

On the Use of Vegetable and Mineral Acids in the Treatment Preventative and Remedial of Cholera, and other Epidemic Disorders of the Bowels. By J. H. Tucker, Surgeon, &c. London: Churchill. 1854. Pamphlet, pp. 32.

Practical Observations on the History, Nature, and Treatment of Cholera Asphyxia. By John Coghlan, M. D., &c. Dublin: Fannin and Co. 1853. Pamphlet, pp. 25.

Statistical Reports of the Belfast Union Hospital, for the Treatment of Contagious Diseases. By Seaton Reid, M. D., &c. Belfast: 1849. Pamphlet, pp. 28.

The Piratical Specific. A New and Infallible Mode of Treatment for the Asiatic Cholera. By Dr. F. Wilson, of Mauritius. London: Churchill. 1854. Pamphlet, pp. 27.

Notes on the Pathology and Treatment of Cholera. By John Rose Cormack, M. D. London: Highley. 1854. Post 8vo, pp. 67.

Experiments on the Communicability of Cholera to the Lower Animals. By W. LAUDER LINDSAY, M.D. (From the Edinburgh Medical and Surgical Journal) Edinburgh: 1854. Pamphlet, pp. 48.

Cholera: An Analysis of its Epidemic, Endemic, and Contagious Character; with Original and Peculiar Views of its Mode of Propagation, and the Means of Counteracting it. Showing also by Analogy that the Means of Preserving Organized Bodies from Decay Point to the only true curative Principles in the Treatment of Fevers generally, and more especially Cholera. By H. STEPHENS, M.R.C.S.L., &c. London: Renshaw. 1854. Pamphlet, pp. 32.

Practical Remarks on the Treatment of Malignant Cholera. By CHARLES Y. HAINES, M.D. Cork: Ridings. 1854. Pamphlet, pp. 40.

Handbook for the Management of Cholera till Medical Aid can be procured. By WILLIAM KINGSLEY, M.D. Roscrea. 1854. Pamphlet, pp. 12.

UNDER other circumstances and at other times, we might, perhaps, hesitate ere devoting a considerable number of our pages to the investigation of a subject, of which we should not be surprised to learn the majority of our readers were already weary. The consideration of cholera is now, however, clothed with double interest, whether we regard the fatality which has attended its recent visitation here, or look abroad to distant scenes where countless victims attest its power. Book after book has been written, while journals, periodicals, pamphlets, and newspapers, have rivalled each other in setting forward, with wonderful perspicuity, the symptoms, progress, and treatment of this disease. Theory has succeeded theory, until human ingenuity, having exhausted the faculty of invention, could only find variety in retracing her steps to commence again anew. Treatment, chemical, mechanical, empirical, and specific, have each been hastily adopted, warmly advocated, and as hastily laid aside, as experience attested not only their insufficiency, but even their inutility. From a perfect chaos of conflicting statements, perplexing dogmas, and pernicious conclusions, truth has in vain struggled to be free. The inquiry arises, why is this? The answer we conceive to rest in the imperfect and one-sided view of disease which many have been led to not only adopt, but to also advocate with the greatest energy. To take a just view of medicine requires a great and comprehensive mind, capable of abstruse inquiry and profound thought.

The object of its study being life as evidenced through death, —the operation of that invisible spirit which permeates the masterpiece of Divine Wisdom,—does man expect that he shall wholly solve the mystery of that organism? If so, let him away with the infirmities which mock his efforts for their relief.

At all times have we cherished a veneration for the healing art, but on no occasion have we been more deeply impressed with its sublimity and responsibility, than when, having perused the several works before us, we paused to ponder on their contents. Manifesting the closest observation and very careful thought, their authors have, in their various suggestions, separate plans of treatment, and different theories, too plainly demonstrated, that as yet it is not permitted us to lift that veil which envelopes vitality; however we may, by the closer scrutiny of its manifestations, more truly appreciate the unseen principles which seem to guide its operation. It is remarkable that, while in their positive observation of this disease a singular consistency and uniformity pervade the generality of writers, yet in their estimate of its causes the widest range of difference is observable. We are satisfied that much of this diversity of opinion is the result of a miscalled "Rational Pathology" which seeks to explain all vital changes by cadaveric appearance, forgetful that these are, but too frequently, merely the effects of causes wholly beyond the reach of the knife. It is very far from our intention to undervalue pathology, or to speak lightly of that school which seems disposed to rest medicine on animal chemistry and microscopy. The elementary constitution and molecular construction of tissues in a state of health or of disease, as indicating more fully the mechanism for vital action, or its evidences, have enabled the physician to accomplish much in preventing, as well as more successfully combating morbid action. All, however, who have most closely studied both sciences in their relation to life will be the first to admit, that, however they may explain changes which have been perfected, however they may recognise the several steps essential for the accomplishment of these changes, they fail in throwing other light on certain internal operations which must have been terminated ere these changes became manifest. Frequently, throughout the pages of this Journal, have we exclaimed against the adoption of principles which would regard the human frame as a machine amenable to ordinary mechanical laws, and capable of being equally guided or governed by principles, which in the abstract sciences are known to prove adequate to the production of certain results. We have also raised our voice against the adoption of a purely vital pathology, which should

exclude all estimation of the reactions and further morbid conditions, which physical changes through their secondary operations are known to occasion. In other words, eclectic medicine has been our theme; which, carefully scrutinizing all particulars derivable from every source, is satisfied that the nearest approach to truth in theory is not that which admits of the most rational explanation, but that which most faithfully accords to our actual observation. Such being the doctrine of our medical faith, we do not hesitate to confess, that general principles, founded on experience, have guided us in the treatment of many diseases, and more especially of that which we are now about to consider. We have long wished for some great and comprehensive work on this disease as evidenced throughout our own country, in which observation and inquiry, on an extensive scale, might be directed by philosophical medical minds. Feeling, however, that the time, labour, and ability, requisite for such an undertaking, might, as regards individual interests, be more profitably employed in elucidating diseases of greater frequency, and more general occurrence, we almost despaired of meeting with so able, elaborate, impartial, and thoroughly scientific a Report as that which Drs. Baly and Gull have drawn up at the desire of the Cholera Committee of the Royal College of Physicians of London. In offering to our readers a necessarily imperfect analysis of the labours of these distinguished physicians, and, as we proceed, in bringing under their notice the chief points of interest the other works we have included present, we are satisfied they will reciprocate the sentiments we have so early ventured to express.

The varieties of opinion as to the remote origin of Asiatic Cholera, and the cause of its spreading, are so numerous that it would be quite impossible to enter into their full consideration, nor should the doing so accomplish any useful scientific purpose, since for this, as for other affections, individuals are found to have propounded and adopted the most contradictory theories. In the Reports of the College of Physicians, Dr. Baly, with that wisdom which his deservedly high reputation insured, has ranged all theories under six principal heads, which may be briefly stated as follows:—1st. That which refers the spreading of the disease to "an atmospheric influence or epidemic constitution," its progress consisting of a succession of local outbreaks, the particular localities affected being determined by certain "localizing conditions," which are, first, all those well-known circumstances which render places insalubrious, and, second, a susceptibility of the disease in the in-

habitants of such places, produced by the habitual respiration of an impure atmosphere. 2nd. That which follows the analogy of diseases known to be due to morbid poisons, and regards the cause of cholera as a morbific matter which undergoes increase only within the human body, and is propagated by means of emanations from the bodies of the sick, in other words, by contagion. 3rd. That which—propounded by Dr. Snow—gives a more specific form to the doctrine of contagion, and supposes that the poison of cholera is swallowed, and acting directly on the mucous membrane of the intestines, is, at the same time, reproduced in the intestinal canal, to pass out, much increased, with the discharges; and that these discharges afterwards, in various ways, but chiefly by becoming mixed with the drinking-water in rivers or wells, reach the alimentary canals of other persons, and produce the like disease in them. 4th. That which assumes that the cause of cholera is a morbific matter or poison, but supposes that it is produced only in the air, not within the bodies of those whom it affects, and that its diffusion is due to the agency of the atmosphere. 5th. That which, a modification of the last, admits that the cholera matter is increased by a species of fermentation, or other mode of reproduction, in impure, damp, and stagnant air, but maintains that it nevertheless is distributed and diffused by means of human intercourse, being carried in ships and other vehicles, and even in the clothes of men, especially the foul clothes of vagrants, and the accumulated baggage of armies. 6th. That which, combining the second and fourth, assumes that the material causes of the disease may be increased and propagated in and by impure air, as well as in and by the human body.

Before determining which of these theories is most in accordance with facts, it is requisite that certain general characters, observed in cholera epidemics, be carefully considered; for unless the hypothesis be borne out by matter of fact, and confirmed by sensible experience, it cannot be true, as being at variance with the realities of nature. We shall, therefore, follow Dr. Baly in his course. The first and most obvious of the general characters of a cholera epidemic is *its unequal and very partial distribution.* In this respect it presents a remarkable contrast to epidemic influenza. Dr. Graves has fully established that while the latter disease in its visitations has pervaded, within a very short period, the whole of Europe, and at one and the same time has more or less involved every part of this island, affecting four-fifths of its inhabitants, cholera has left whole districts unvisited, and has fallen severely on comparatively few localities. This remark respecting the eccentric and

irregular course of cholera, as regards the country at large, equally applies to its more limited visitations; for, if attention be fixed on single towns, isolated streets, or public institutions, the same character is observed. One part of a town suffers most severely, another escapes altogether. In a few houses in a street half the inhabitants are carried off by the disease; in the remainder not a single death occurs. In a large public establishment, such as a barrack, a lunatic asylum, or a prison, it often happens that the disease is, at least for a time, confined to one wing of the building, one ward, or one series of rooms. These are facts which, to use the words of Dr. Graves, are "of considerable weight in arguing whether cholera, like influenza, is propagated by atmospheric influences." The second general character of a cholera epidemic is, that *the localities, especially and most severely, visited by it, have certain features by which they are distinguished from those other places which entirely escape or suffer only in a slight degree.* In this proposition the several authors before us fully agree. It was observed that the parts of England in which the rate of mortality was highest in 1849, as well as in 1832, were, with a few exceptions, the more-densely populated regions lying around great rivers, on the sea-coast, or in the neighbourhood of mines, especially coal-mines. In this respect a close analogy may be traced between cholera and yellow fever, which latter disease Dr. Cummins, in his highly valuable papers on the subject, has observed to prevail to the greatest degree either at the coal depots on the West India station, or in those ships moored next to them. The preference of cholera for such localities is shown by the fact of its having been three times as fatal in the registration districts on the coast than it was in the interior of the country.

The conditions most generally present, wherever a considerable mortality occurred, may be stated as being represented by a low, damp site, and a crowded population with defective ventilation. The experience of Irish physicians is in perfect unison with this fact, which at once suggests the inference, that the morbid cause, whatever its nature be, finds the conditions for its increase or for its action, at least in part, in the impure atmosphere of such places. Dr. Baly, applying to these facts that theory of epidemic cholera which attributes it to a "general state of the atmosphere," or "atmospheric influence," brought into action by "localizing causes," observes:—"Such a cause would be expected to produce its effects wherever the localizing conditions exist." Yet the Report declares, though it has been shown that the epidemic proved most violent and fatal in localities of a certain nature, it is also undoubted that very many places,

some towns, many smaller places, and parts of towns, presenting the same condition, have suffered but slightly, or wholly escaped. From facts of this nature it is justly inferred:—If the cause of cholera be one generally present through the atmosphere, there must be some other condition essential for its action besides the known conditions of its insalubrity; and this unknown condition must, in some instances, have been absent throughout entire towns; in others, only in limited spots. The partial distribution of the epidemic, and its absence from some of the places presenting the conditions favourable to its development, argue strongly in favour of the theory of a "cholera matter," not equally diffused through the atmosphere, but only partially distributed and transported in some way or other to the places affected. These facts are likewise considered as leading to the further inference, that if the cause of cholera be a material poison, it has most probably not a gaseous form, since a gas soon becomes diffused through the air and dissipated, while the cause of cholera remains many days producing its effects in one limited spot; while, lastly, the close relation in which the intensity of the epidemic seems to stand to density of population, and activity of traffic, though not inconsistent with any one theory, especially suggests the probability of the disease being in *some way propagated by human intercourse.* All honour to the memory of Robert James Graves, whose master-mind, many years since, with an elaborateness of research and an accuracy of inference pre-eminently his own, traced the progress of this disease throughout the world, and thereby demonstrated that where the epidemic spread from one country to another, *it never traversed the ocean at a rate exceeding that of ships.* While further observing that, in its travels up the highest mountain passes, as in India; or across the ocean, as to the Isle of Bourbon; or when accompanying the caravan across the Desert, as when it arrived at Mecca and Medina; or when ascending rivers, and making the towns on its banks the successive stages of its journey; "*in all such cases cholera seemed regulated by no common physical circumstances except human traffic and human intercourse.*"

The third character of an epidemic of cholera is, *its long duration in a country, or even in a town of large size.* This character of cholera may be regarded as distinguishing it from other epidemics, more especially from that of influenza, the duration of which in one town does not generally extend beyond a few weeks, and in a whole country seldom exceeds two or three months. The theory which attributes cholera to a general state of the atmosphere, or atmospheric influence, might be

fairly presumed to argue a certain relation between the direction of the wind and the degree of prevalence or intensity of the epidemic. Observation has proved that no such fixed relation exists. To this subject we shall presently have occasion to again refer, when noticing the able and deeply interesting Report of Sir William Burnett. Many arguments, based on the continuance of the epidemic in one locality, and on the fact that changes in the wind do not sensibly affect the progress of cholera, tend to ignore the proposition that this disease is *primarily* due to a general state of the atmosphere, or to an influence moving with the atmosphere. The theory which assumes the dependence of cholera on a morbific matter, transportable within limits from place to place by the atmosphere, and capable of increase, under favourable conditions, in the places to which it is conveyed, is regarded as affording a more simple explanation of the long continuance of cholera in the countries or towns which it visits.

A fourth and important character of cholera is, *the intensity of an epidemic of the disease varies during its continuance in a country or a large city, so that it has periods of little and of great activity, and usually well-marked periods of increase, acme, and decrease.* Before proceeding to estimate the import of this character of the disease, with regard to the principal theories of its cause, it is requisite to inquire into the external circumstances or conditions which attend and apparently determine the increase and the decrease of its prevalence in any place where it exists. The first inquiry which here arises has reference to the period of the year in which cholera has been observed to prevail with the greatest intensity; and secondly, how far meteorological influences conduce to its greater prevalence at those periods. Amongst the thirty-two epidemics, of which particulars are in the Report before us set forth, six produced the greatest mortality in the month of July, seven in August, and seven in September, twenty in all, in one or other of these three months. And amongst thirteen epidemics in the chief cities of the several countries (New Orleans and New York being taken as the capitals of the Northern and Southern States of America respectively), eleven were most fatal in one or other of the same months, namely, three in July, three in August, and five in September. That the season has no exclusive influence in determining the time of culmination of the epidemic is shown by the fact, that of the thirty-two instances, three epidemics were most fatal in June, three in October, one in April, two in March, two in January, and one in December; while in the same city the greatest mortality from the disease has, in different epidemics, occurred in different months. Paris, for example, in

1832, suffered most severely in the month of April, and in 1849 in the month of June. That temperature has a large share in regulating the severity of the epidemic is probable, from the more frequent prevalence of cholera in an intense degree in the latter months of summer and the beginning of autumn, as also from the fact that it is much more fatal in hot than in cold climates. Yet, as other facts tend apparently to an opposite conclusion, it is of importance that those matters be investigated, which may assist us in determining the connexion really subsisting between the epidemic and temperature. It is a matter of general observation, that when epidemics of cholera have begun during the hot season, they have quickly risen to their climax, and have then soon declined and become extinct on the advance of the cold season; whereas, in the instances in which they have commenced at the close of the year, they have, as a rule, not caused any considerable mortality before the following spring or summer. There is, notwithstanding the arguments in favour of high temperature as developing or increasing the virulence of the epidemic, abundant proof that the increase of cholera is not necessarily connected with any particular degree of external heat, nor its decline with the diminution of that temperature. This is, by Dr. Baly, set forward through several tables and diagrams illustrative of the relation between the mortality from cholera and average temperature as observed in London, Paris, and Berlin. These Tables oppose a new set of facts to those already quoted; for, while previously admitting that temperature, as a rule, does in some way or other exert an influence over the degree of prevalence of cholera, we are now satisfied that this influence is not the only one capable of producing the like results, and is neither necessary nor constant in its operation. The marked preference of cholera for those localities in which the conditions of insalubrity abound, may tend to explain the nature of the influence exercised by temperature, and the probable reason why, in many cases, it is superseded. These conditions are believed to act principally by the production of an impure atmosphere. The same influence, acting on various conditions, must produce corresponding variations in those conditions. It is, therefore, obvious, that the higher the external temperature, the greater will be the amount of impurities resulting from the decomposition of organic matters in the atmosphere, which renders it highly probable that the usual influence of a higher temperature, in increasing the intensity of a cholera epidemic, consists in adding to that impure condition of the atmosphere, which so much favours the local development, and probably

also the diffusion of the disease. Other meteorological conditions are found to aid a high temperature in producing an impure state of the atmosphere. Great stillness of the air causes a localization of its impurities; a certain degree of moisture promotes decomposition, and tends to the same result. When an increased temperature is superadded to these, the most favourable combination of circumstances, which predispose to cholera, may be presumed to be present; since the probability is, that a high temperature favours the growth of a cholera epidemic by tending to increase the impure condition of the atmosphere. In opposition to this presumption certain exceptional cases arise, which lead to a doubt that the same conditions which determine the degree of severity of the disease in particular spots, also regulate the variations in its intensity as an epidemic at different periods in the same place. Some of those villages in which the winter outbreak of cholera, during 1848–9, proved most severe, were not by any means distinguished for their local attractive causes; while in other instances, where the local conditions which are believed to generally favour the increase of cholera, were abundant, the epidemic, which had been severe during the cold season, declined with that change of temperature which might be presumed to favour its continuance. These facts, while maintaining the necessity of the closest attention to all sanitary measures, at the same time lead to the belief that some unknown cause exists, which exerts an important influence over the degree of prevalence of cholera. So far the inferences which these observations warrant though affording no grounds for positive assertion, yet indicate such precautionary measures as could not fail to be of great general advantage, and which might also be reasonably expected to exercise much particular influence on the progress and character of this disease.

The variations which have been observed in the intensity of cholera epidemics, and the circumstances which determined these variations, are considered by many as not favourable to the theory, that cholera is propagated and diffused by means of human intercourse. These facts are regarded as tending to establish a close resemblance between Asiatic cholera and the common summer cholera and diarrhœa of this country, and remittent fever, which there are such good grounds for believing to be purely of malarious origin, and not to be in any way communicable. The question of the contagion or non-contagion of cholera, is one so nicely balanced, that hosts of startling facts might be adduced in support of either side. Allowing it to be contagious, we are bound to admit that it differs from the ordinary contagious

diseases of this country, in being far more under the influence of temperature, and other conditions of the atmosphere, and that its diffusion through a town takes place with much greater rapidity than that of any known mechanical agency. If we altogether ignore the influence of contagion, we are quite unable to respond to the arguments advanced by Graves, or to explain the cases detailed by Dr. Seaton Reid, of Belfast, who, in the able pamphlet with which we have been favoured, adduces such evidence as affords just reasons for believing that cholera is in some way propagated by human intercourse. "Two questions," Dr. Reid writes, "naturally suggest themselves in relation to contagion in this disease:—1st. Is it necessary that every patient who suffers from an attack of cholera should have been within the sphere of contagion?" To this Dr. Reid replies, "that it is not." 2nd. "But, if a case has occurred, will those who are near the patient, either by living in the same house, or being called to attend him, be more likely than others not so circumstanced, to take the disease?" Dr. Reid's ample experience compels him to reply, "that they most certainly would." The fact of conflicting opinions existing on matters of observation, testifies that the data for judgment have been either imperfectly, or erroneously estimated. Yet, on this matter, we incline to the supposition that neither are wholly wrong, since no *simple* theory will account for all the facts in the history of epidemics of cholera.

The view of the cause of cholera which seems most consistent with the variations of intensity of a cholera epidemic, and with what is known of the circumstances determining those variations, and which at the same time accords with the other characteristics of cholera previously considered, is the "miasm" theory,—that, namely, of a material substance distributed through the air, and undergoing increase in the air, or on surfaces exposed to the air. This theory would be perfectly consistent with the observed phenomena which are favourable to the extension of the disease, as the "miasm" might be supposed to find its pabulum in the increased impurities of the air, while the exceptional phenomena, which occur in the course of cholera epidemics, might be assigned to the action of an unknown cause, capable of destroying either the morbific miasm itself, or the impurities of the air. This theory, however, according to observation, far from, to our minds, ignoring the doctrine of contagion, is a strong argument in its favour, as its negative propositions rest on the recognition of a presumed, though unknown, collateral agency.

The fifth character of cholera is, that *after a certain time it*

altogether disappears. Though the cessation of cholera usually occurs in the cold season, cold, while exerting a suppressing influence, is as certainly inadequate to, of itself, eradicate the disease; for the epidemic has often survived a severe winter in colder climates than that of England. It is, therefore, but just to infer, that some other cause must at least concur with the influence of cold in bringing about such a result. The many who advocate the dependency of cholera on contagion, or, in some way or other, on human intercourse, regard this peculiarity of the disease as being quite consistent with their theory, explaining its cessation by the assumption, that all persons are not susceptible of the disease, which, therefore, ceases in any locality when those inhabitants, who are so, have been exposed to the contagious influence, and affected by it. In corroboration of those views, which regard cholera as essentially contagious, Dr. Snow, and others, have adduced the fact, that, in the epidemic of 1832, a direct relation subsisted between the number of the population, and the duration of the disease in different towns and villages. Dr. Snow thus writes:—

"If the cholera cases were not connected one with another, there would be no reason why the few cases which happen in a village should not be scattered over as long a period as the thousands which occur in a great metropolis."

Here, as in many other points of dispute respecting this disease, facts tell both ways. We have admitted that certain localities manifest an aptitude for this disease. Such localities are not simultaneously affected; as those localities are more numerous in large towns, it is clear that in whatever way the successive attacks of the different localities were produced, a longer time would elapse before the disease could run its course through the more populous and more extensive place. Arguments against the contagion of cholera are presumed to rest in the fact, that the disease ceases in public institutions and other such places, while numbers susceptible of its influence continue unaffected; while the supposition that cholera, if contagious, ceases throughout a country or city from the absence of any more persons susceptible of its influence, is at variance with our experience of other contagions, which, notwithstanding their subsidence, are never wholly extinct. While these facts argue that cholera may be communicated from one person to another, observation leads to the inference, that it is at least improbable that the cessation of the epidemic throughout the country, or even in a town or village, is due to the failure of subjects for the contagious influence. On this point we quote

from the able Report of Dr. Balfour, who, having dwelt on its influence on different races of men, and the greater susceptibility of Europeans to its attacks, adds:—

"It is possible, indeed probable, that several points which have hitherto been deemed inexplicable, connected with the manner that cholera developes itself, may be cleared up by the knowledge of this single fact; particularly the many instances which have occurred of regiments, when marching through districts perfectly free from cholera, being attacked with the disease in a virulent epidemic form, and losing a great number of their soldiers; the possibility being that the agent, although present in the district, has no power over the less susceptible native inhabitants, but exerts its full influence on the stranger races of whom the regiment is composed; and the fact of two native regiments marching at the same time, along the same route, and through the same villages, and crossing each other on their journey, the one regiment suffering severely from cholera, while in the other regiment not a single case occurs, may likewise be referrible to the same law."

This observation of Dr. Balfour, while clearly tending to support the view of an individual liability to the influence of this disease, may be responded to by another truth equally remarkable, that cholera, having fatally prevailed in a localized community, for a time disappears, and again returns to break out with greater violence amongst those previously unaffected. As an argument against the contagion of cholera, or against individual liability, occurrences of this nature must be received as but negative proofs, for the same peculiarity of action is not unfrequently manifested by other diseases of an admittedly infectious or contagious nature. The mere circumstance of one or many persons escaping the attack of a disease, known to be contagious, proves no more than this,—at the period of such exposure their physical aptitude was not favourable to the reception of the morbid agent, and affords no further grounds for the affirmation, that, under other circumstances, very different results would not have ensued. If any exemplification of this be wanting, we refer to the fearful mortality which occurred amongst Irish medical men during the famine years, when mental and physical depression combined to render them so many victims to the discharge of their public duties.

The entire disappearance of cholera, regarded alone, is most readily explainable by the theory, that the epidemic is produced and maintained by a peculiar state of the atmosphere, or atmospheric influence, which at length passes away, and, of course, leaves the country free from the disease. According to this view, fresh outbreaks would involve the necessity of

supposing fresh visitations of atmospheric influence. If, on the other hand, the view be adopted that cholera depends on a material poison conveyed by the atmosphere, but only partially distributed through it, this poisonous matter increasing wherever there are the conditions of damp and foul air, the cessation of the epidemic must, it would seem, be ascribed to some other cause, which either frees the entire atmosphere from matters essential to the existence and increase of the cholera poison, or destroys the poison itself. Ozone, which has been shown by Professor Schönbein to exist in the atmosphere in varying quantities, has the power of destroying the impurities resulting from the decomposition of organic matter, and has so far the requisite attributes of this unknown agent. Beyond this fact we can argue nothing; for were we to admit that the electricity of the atmosphere bears any fixed relation to cholera, we should ignore recorded observation; and were we to infer that variations in the atmospheric proportions of ozone eventuate in either the production or continuance of cholera, it would follow that all diseases which are fostered by an impure air ought to be equally under its influence, which experience proves is not the case.

The sixth character of this disease Dr. Baly enters on, is *the manner of its dissemination as regards time; that is to say, the degree in which its appearances, its period of greatest intensity, and its cessation were severally simultaneous in different places.* It is scarcely requisite we institute an analysis of those elaborate tables from which Dr. Baly draws his conclusion in reference to these points. It is sufficient for our purpose to observe that they fully corroborate the several statements we have already set forth; the general results to which they lead being, that in the spring and summer of 1849, different towns began to be affected in succession and not simultaneously; and that the order in which they felt the effects of the epidemic was, as a general rule, connected with certain characters of site, density of population, and trade. Towns lying on, or near the sea coast, or on navigable rivers, and consequently on a low level, having at the same time a dense and poor population, with a large accumulation of ill-constructed and ill-ventilated dwellings, surrounded by all kinds of impurities, being those which were usually the first affected, the same localities, as might have been inferred, also presented the greatest mortality. Exceptional cases are, however, not wanting, which prove that the obvious local conditions which are generally present in tracts of country and towns, rarely attacked by the epidemic, either do not in all cases and at all times include the essential ele-

ment which determines the manifestation of the disease in a given place, or are liable to have their influence counteracted by some other agency. Though when cholera is established, localities, having the obvious features of insalubrity most decidedly marked, seldom escape its visitation, it may occur that the spots it first visits are not those which present the characters of unwholesomeness in the most marked degree. This is further confirmatory, that obvious conditions of insalubrity do not necessarily include all that is essential for determining the spots in which an epidemic of cholera shall produce its first effects. The investigation of the relation that existed between the outbreaks of cholera, in different places, in regard to the *periods of their greatest intensity*, while affording some ground for the belief that, even in the winter, the intensity of the epidemic was, in a measure, regulated by a general influence, at the same time shows that the general tendency of the epidemic to reach its climax in August or September, and perhaps the influences producing that tendency, were, in the instances of many towns and registration districts, counteracted or interfered with by special care.

A careful investigation of the progress of the epidemic in each district, leads to the opinion that the general result of an augmented mortality at a particular time was due, rather to cholera affecting a larger number of individual spots, than by its producing an increased mortality in all the localities previously visited, as also that it appears to have exerted its fatal influence in the several seats of its chief action, in succession, not simultaneously.

As a general rule it may be stated, the towns, in which the disease tarried latest, had all the obvious characters of site, density of population, and sanitary condition, which distinguished the towns earliest and most severely visited; and in a large number of instances, the places that suffered longest were those which had been earliest attacked.

There are, however, many exceptions to this rule, or, at all events, many facts relating to the time of cessation of the epidemic in different places, which cannot be thus entirely explained; but on the whole, these cases are less remarkable than those of the long persistence of the disease; while both series of instances present decided exceptions to the rule that the early cessation or late persistence of the epidemic, in different places, was determined by the degrees in which they severally presented the known local conditions of unhealthiness. It is true that in certain exceptional cases, the mere local conditions, regarded as sources of foul air, do not account for the

sudden occurrence of some of the outbreaks after the epidemic had subsided elsewhere, and for the continued absence of the disease from others of them, after the first early outbreak had ceased, although the epidemic was increasing in other places. Such facts, while having their particular value, do not materially affect the position, that impure air is an essential local condition for the severe manifestation of cholera; in other words, the principal condition, by virtue of which individual localities, and the persons inhabiting them, are susceptible of the influence of the epidemic.

We could not adduce a stronger argument in favour of the doctrine of contagion, and the influence which foul air, and association with those affected by cholera, are capable of exercising, than that which Sir William Burnett furnishes in his Report, containing, as it does, heart-rending accounts of the fearful havoc cholera has occasioned amongst the fleet. Having remarked that diarrhœa and dysentery are of frequent occurrence amongst the inhabitants of the low damp valleys of the Bulgarian and Wallachian provinces during the summer months, but that they do not assume the choleraic form, which appears to depend on a cause not endemic, Sir William observes:—

"But whatever the cause, or causes, may have been which gave rise to the disease first in the allied camps, and secondly in the fleets at Baljick and Varna, there is this remarkable fact to be noticed, that it did not occur, at all events, in an epidemic form, *until vessels had arrived from an infected port, with men on board actually ill of the disease at the time of their arrival.*"

In a Report received from Mr. Deas, the Deputy-Inspector of the fleet, he mentions, on the authority of Dr. Hall, Inspector-General of Hospitals attached to the army, that a French officer, who had just arrived from Africa, died of cholera at Gallipoli on the 3rd of June; and that another case proved fatal to a soldier of the 19th Regiment, on the 17th of June, at the British encampment at Alladeen, some twelve miles distant from Varna; but that no other case appeared in either army for some time. These cases, from their isolation and the circumstances under which they occurred, were regarded as cases of the sporadic form of the disease. It was not until about the beginning of July, when the 5th French Regiment of the line arrived at Gallipoli in a steamer from Marseilles, where it is believed cholera existed at the time of her departure, that the disease can be said to have thoroughly declared itself. Mr. Deas mentions that—

"It broke out on board the vessel during the passage, and four or five men died of it. Soon after the arrival of this regiment at

Gallipoli some divisions of the French army marched for Varna and carried the disease with them; these divisions, it was stated, though not on good authority, lost twenty men on their march.

"But it is quite certain that cholera made its appearance at or near Varna soon after their arrival; at first it was confined almost exclusively to the divisions which had come from Gallipoli, but it afterwards spread extensively through the camp."

On the 21st of July a decided case occurred in the British hospital at Varna: the disease then spread rapidly to the men outside the walls in camp, though "*the majority of the first eighteen attacked were at the time patients under treatment in the hospital for other diseases.*" On the 22nd the disease is reported as breaking out with great severity in the camp at Devina, which is about sixteen miles distant from Varna, but not more than four from Alladeen. The Report further adds:—"Free communication had been kept up all the while between the different British and French camps." Why, then, it may be asked, are we at liberty to infer that the French communicated the disease to each other, and with this free communication failed to at once do so generally? It is questions of this nature which afford grounds for different opinions.

During the months of May and June the British fleet appears to have enjoyed excellent health; but towards the latter end of June, and early in July, diarrhœal complaints became more numerous than usual when the vessels approached the land, but more particularly when they anchored off either of the ports of Varna or Baljick. It is believed that those attacks were rather of an endemic than of a choleraic character. According to the medical returns the "Diamond" was the first vessel in which the epidemic made its appearance. She had anchored on the 7th of July at Baljick, in shore of the line-of-battle ships. Her crew were in perfect health; but on the 9th, 12th, and 13th, cases of diarrhœa, accompanied by a prostration of the vital powers, occurred amongst them; and on the 14th of July a French steamer, the "Primoguet," arrived from Toulon, and, as several of her crew were affected with cholera, she was sent in shore of the English squadron to be cleared out, and to undergo a sort of quarantine. The weather at the time was tempestuous, and *the wind blew towards the "Diamond" from the position of this steamer and her tents.* On the night of the 16th one of the "Diamond's" crew was attacked with collapse, rice-water purging, vomiting, cramps, and the other unequivocal symptoms of cholera; but it was not followed by any other case of a similar nature until the 20th, when diarrhœal attacks began to be prevalent, though they again

speedily disappeared when the ship anchored further off the shore. This ship, we are informed, remained at her new anchorage until the 11th of August, and had, in the mean time, the greatest attention paid to her sanitary condition; she then went to sea with several vessels in which the disease had broken out in a most virulent form, yet, though she remained close by the latter, there did not occur another case deserving the name of cholera amongst her crew until the end of September, when there were two slight cases, which appear to have been contracted somewhere off the Crimea.

This, and the following statement, are, perhaps, the best, as they are the most recent confirmations of many of the circumstances and peculiarities of cholera mentioned throughout our review. We pass over the eruption of the disease at Varna, as no matters of pathological interest appear to have been associated with it.

We read that on the 9th of August, the disease in its malignant form broke out simultaneously in the "Britannia," "Albion," "Furious," "Trafalgar," and "Tribune." The first of these we shall particularly notice. It appears from the report of Mr. Rees, Surgeon to the "Britannia," that when, on the 30th of July, this ship encountered the epidemic, she was, as regards crew and general arrangements, in a most favourable condition to do so. The first days of August were oppressively hot, and a dead calm generally prevailed throughout the bay. Diarrhœa sprang up among the crew, and on the 9th, one case merged into cholera. On the morning of the 12th the ship drew out of the bay, in her passage getting the advantage of the prevailing north-east breeze. The patients did well on that day and on the following. On the night of the 13th the breeze freshening, the lower deck ports had to be closed, but the ventilation by means of wind-sails was maintained to the fullest extent *practicable by such imperfect means*. On that night, and towards the following morning, a fresh case of cholera with collapse occurred; but the applicants with diarrhœa were not more numerous than they had been during the previous night. As the day advanced, however, there was a marked change for the worse in all the diarrhœa cases; one of these patients, as he walked into the sick-bay at the morning visit, fell into a state of collapse, whilst three other men presented themselves who appear to have approached the very verge of cholera.

"About 10, A.M.," Mr. Rees states, "the great outbreak commenced, but as a minute description of the scene which followed would be foreign to my purpose, a few bare facts and figures must be left to tell the tale of an invasion which, for the suddenness of

its advent, the tempest violence with which it raged, and the wreck it left behind, has surpassed, perhaps, everything of the kind in the annals of the service.

"I have mentioned," continues Mr. Rees, "the marked alteration in the cases of diarrhœa observed on the morning of the 14th; this unfavourable change might be attributed to the closed ports and consequent defective ventilation, but being at sea, one entire watch was of course kept constantly on deck; it is therefore difficult to account for the outbreak which took place on this fatal day; although some of the seamen and officers have since spoken of a peculiar dense cloud which passed over this ship and the 'Albion,' on the 13th.

"The first to be attacked were the men already on the list for diarrhœa, several of whom fell into a state of collapse one after the other; but about the same time, robust, healthy men, who had fallen suddenly down in a state of collapse, began to be brought in from various parts of the ship, even from the yards, where they were seized while reefing sails.

"The duties of the medical officers become now almost overwhelming; men, struck down in the manner described, were brought in so frequently and in such numbers, that the senior assistant and myself had to devote our entire energies to such pressing cases only, whilst the other assistant had to stand at the table in the sick-bay, dispensing medicines to a stream of men complaining of diarrhœa, both real and imaginary, for a panic had now seized the crew. This scene continued for many hours; and it was not until the following morning, when at length the panic became less, that the names of the patients living and dead, could be inserted in the sick-list.

"From the 14th to the 17th, both days included, there occurred 201 cases of cholera, and 93 deaths. The experiment of putting to sea having in our case failed, the propriety of returning to anchorage was determined upon; but we had, in the meantime, been driven to leeward by a strong north-east wind, and there was, consequently, an inevitable delay in getting back to Baljick.

"The necessity of return to port become daily more evident, by the continued violence of the scourge, and the now crowded state of the middle deck, where the discharges from the bowels and stomachs of the sick, and the want of adequate ventilation, had contributed to render the ship at length, in my opinion, the laboratory of pest-poison.

"On the 17th we gained the roadstead of Baljick, and finding the place full of empty transports, all the sick and the healthy, with the exception of officers, sentries, and two boats' crews, were transferred to them, the bad cases being selected for the 'Apollo.' Only twenty-two cases of cholera occurred afterwards, *and nearly all the victims were men who had been in close attendance on their sick mess-mates, and had accompanied them to the transports.* There is one gratifying circumstance, however,—the disease was not communicated to the crews of the latter."

We have deemed it advisable to give this extract in full. It affords much field for reflection. The total number of cases of diarrhœa which occurred during this terrible visitation, amounted to 396; the total number of cases of cholera to 229, of which 139 terminated in death. The total number of men borne on the ships' books, when the disease broke out, amounted to about 1040, so that the loss was about 13 per cent. of the whole crew.

We shall not follow the details of the mortality in those other ships, wherein so many brave and gallant fellows perished, but conclude our notice of this report, by extracting the following observation of Sir W. Burnett.

"In the vessels which lost many men, the mode of treatment adopted appears to have been as judicious as it was in those that lost but few; the rate of mortality being influenced, apparently, much more by the violence or concentration of the exciting poison than by any other cause. It is satisfactory to observe that no remedies of an heroic, of a dangerous, or of a purely empirical nature were employed, with the exception of such of the latter as had been sanctioned by previous use. The results on the whole, as regards the medical treatment, were not satisfactory, but they were not more disastrous than generally happens from similar visitations."

The exact nature of the part which may be played by the atmosphere is not always very obvious; many instances may, however, be referred to in which it seems to be apparent that the atmosphere, if it at all participated in the production of the results, most probably served as the means of communicating or transmitting the cause of the disease from one locality to another. Facts leading to this inference rest on the investigation of the successive mode of attack of individual localities, houses, groups of houses, and public establishments, within the same district, which seems to argue that the impure atmosphere of a district determines the occurrence of numerous outbreaks within its area, by rapidly communicating the disease from one locality to another. There are good grounds for believing that, in certain states of the atmosphere, a body of damp and impure air may travel over the surface of the country for many miles, still preserving its original properties. It is quite possible, therefore, that such air, if capable of at all transmitting the disease or its cause from place to place, may convey it from one town to another some miles distant. The recognition of such a fact in our general estimate of this disease, by no means militates against the adoption of opinions with regard to its communicability by other, and less universal means, whatever the nature of this atmospheric poison

be. It is important to know that all the facts relating to the extension of cholera render it probable, that the unknown condition of the air, which represses its progress, is most frequently associated with comparative dryness and purity of the atmosphere, and the unknown condition which promotes it with the presence of moisture and impurities. The lingering of the disease in certain spots, for the most part distinguished by the local conditions productive of a damp and impure air, while elsewhere throughout the country it had disappeared, is in accordance with the theory of a poison which finds the means for its maintenance and increase in such places and in such an atmosphere. The renewed rise of the epidemic in the summer might be reasonably referred to the increase of impurity and moisture in the air under the influence of rising temperature, and perhaps other meteorological conditions, to the consequent increase of the poison in the localities where it already existed, to its distribution with the air from these different foci to other places more or less distant, to its increase in these again, if they afforded the necessary conditions, and to its further diffusion from them to other places, through the medium of the atmosphere. The order in which the different parts of the country and different towns were attacked, and ceased to suffer during the year 1849, further favours such a theory. Again, the climax of the epidemic being nearly simultaneous in the different localities visited by it, at the season and in the area, most remarkable for impurities of the atmosphere—a fact which the tables of the London registration district as set forward in this report assert—though suggesting the inference, that impure air was the medium through which the disease or its cause was communicated from one spot to another, yet is, for obvious reasons, much more intelligible on the assumption that the morbific matter was diffused by currents of air.

The theory that the cause of cholera is a general state of the atmosphere, an "atmospheric influence," or "epidemic constitution" of the air, is untenable, if this general state is referred to the moving mass of the atmosphere. Since, while the air is constantly in motion, and changed by the currents setting in from different quarters in succession, the epidemic continues for a long time persistent. We may observe that the term "epidemic constitution," as here quoted, is far different in its signification from that which its adoption by Sydenham implied; as now used, it refers to the atmospheric aptitude or tendency to one particular disease, its original signification or intention being to express a certain otherwise inexplicable

universality of type, which at times is observed to appertain to all diseases.

A modification of the theory of "an atmospheric influence" supposes a power operating in the atmosphere, though not moving with it, and producing its effect on the human body in localities where the atmosphere is foul, or in persons rendered susceptible by long dwelling in an impure air. This theory includes the vaguely propounded notions of a particular "electrical state," and a "telluric influence."

The theory that cholera is diffused by means of human intercourse affords a ready explanation of many of the more remarkable circumstances relating to the epidemic, which point rather to a morbific poison, partially distributed, than to an agent or influence existing throughout the atmosphere, as, for instance, the early appearance of cholera in sea-ports, and in countries and towns where traffic is active, its appearance in the same country not simultaneously, but successively, the late attack of some places, which, for site and sanitary condition, seemed obnoxious to an early invasion, the successive mode of attack of different localities in small towns, and the appearance of the disease in small places around great foci of the epidemic. Diffusion by human intercourse seems to be by no means the most appropriate explanation of other facts of great importance in reference to this disease.

In relation to these adverse facts, a distinction must be made between the two principal modes in which human intercourse may be the means of propagating cholera. The idea usually associated with the diffusion of a disease by human agency is, that the disease is the effect of a virus which is reproduced in the bodies of the sick, and which, emanating from their bodies, infects other persons; and consequently, that a person receiving the contagion in one place, and travelling to another, may convey the infection thither in his own body. The close connexion between cholera and local conditions, the great influence exerted over the epidemic by atmospheric changes, its want of analogy with other contagious disorders, the rapidity of its spread through large cities, with various facts relating to its commencement, climax, and cessation, in different places, appear to furnish strong reasons for regarding the theory of direct personal contagion with something more than doubt. There is, however, a second mode in which human intercourse might be the means of communicating and diffusing cholera; a poison, undergoing its increase out of the body, might be carried from place to place in the vehicles of men, in their baggage,

or even in their clothes. This view is believed to be consistent with the history of the dissemination of the epidemic, not only in all those particulars which we have mentioned as favourable to the idea of its diffusion by human intercourse, but also in respect to the power exercised by the sanitary condition of different localities, and especially by the state of air in them on the extension, as well as on the local severity of the epidemic; since, according to this theory, the poisonous matter, though carried to many places by human intercourse, would only produce and increase the disease in spots where the air is foul. Observations are not wanting which militate against this theory as an exclusive one. That human intercourse has a large share in the diffusion of this disease is no argument against the propotion that other agencies are capable of exercising even a greater influence. The manner of the commencement, progress, and cessation of cholera in each particular spot visited by it, has been inferred from its progress and duration in the several lunatic asylums in England, during the epidemic of 1848–49, from which it appears that the commencement of the disease, usually, in one limited part of a public establishment, or in one of a group of houses, and its extension to others in succession, is not easily reconcilable with the theory of a general and persistent atmospheric state or influence; a much more probable explanation of the commencement of the epidemic in one part of an asylum or other public establishment, and of its subsequent extension through other parts being afforded by either of the theories which supposes the cause of the disease to be a material poison, transferable from spot to spot, and from person to person, by human intercourse or by currents of air.

The theory of the production of cholera by a general atmospheric influence fails to explain the localization of the disease where circumstances are apparently equal, except on the supposition that a local condition adequate to excite the general atmospheric cause to action exists.

The doctrine of pure contagion, while explaining some facts, would fail to solve others of equal importance. If the cause of cholera be regarded as a poison, not reproduced within the bodies of the sick, but capable of independent increase and existence, its propagation is easily accounted for, while its cessation might be safely ascribed to a process of self-exhaustion, or to its destruction or removal, either by an altered state of atmosphere, or by the ventilating, cleansing, or other sanitary processes adopted. Under such circumstances the extinction of the epidemic would be the conjoint result of the limited dura-

tion of the disease in each spot visited by it, and of the supervention of a condition of the atmosphere no longer favouring its communication to new spots.

In noticing the manner and progress of cholera across a continent, or across a sea, from one country to another, the general laws which appear to regulate its diffusion are in complete accordance with some of the main features presented by the epidemic in this country, which, as has been observed, furnish strong grounds for the belief that the state of the atmosphere has a paramount influence over the extension, as well as the degree of local intensity of the disease, and obviously favour the theory of a material poison conveyed from place to place, increased or reproduced in those places in which it finds the necessary or favourable conditions, and thence transmitted to other places; its transmission, as well as its increase, being promoted by damp and impure air, and by warmth, and being impeded by a dry and pure air, and by cold. Many notorious facts relative to the transit of cholera from one continent to another, and to and from an island, concur in affording strong presumptive evidence of its connexion, in some way or other, with intercourse by shipping. The transit of cholera up rivers, along the course of roads, in direct opposition to the prevailing winds, or, to use the words of the Report, "in the teeth of the monsoon,"—the fact that its rate of travel is usually progressive with that of men,—constitutes strong presumptive evidence of the diffusion of the disease by human intercourse. While, again, many cases might be cited in which localities become affected, where no such cause could be traced or probably existed.

Many of the grounds on which the strictly contagious nature of cholera is usually maintained, merely tend to show that the disease is communicable by human intercourse. The special and direct evidence in favour of the contagious nature of cholera consists of three classes of facts. The first is, that in a vast majority of the instances in which the introduction of the disease into a town or smaller community has been traced to human intercourse, the seeming vehicle of the infection has been a person or persons actually infected with the disease. The second, that persons who have washed or handled the clothes, linen, or bedding, of cholera patients, have frequently themselves become affected. The third class of facts is constituted by the attacks of the disease experienced by nurses and other attendants of the sick. The Report before us furnishes several conflicting arguments in reference to the contagious nature of cholera derivable from observation on these three points, which,

though generally conclusive of the fact that the disease is communicable by human intercourse, at the same time only show that the human body forms one nidus for the reproduction of the poison; that contagion bears but a part in the propagation of the epidemic; that comparatively few persons are susceptible of its influence; and that by proper sanitary precautions it may be almost entirely disarmed of its power. There is, as has been already mentioned, reasonable ground to infer that the cholera poison is, under certain circumstances, wafted from place to place by the wind. Its appearance in ships, anchored at a distance from an infected shore, and in the course of the land-winds, its general appearance throughout a town, the mode of its extension through it, as well as the origin of the disease in many public institutions, is inexplicable on other suppositions. It does not, however, follow, that the cholera poison should retain its properties when carried by winds over distances of very many miles, it being, as a general rule, more probable that it would in a long transit be dissipated and destroyed by the purer air with which it would be brought into relation.

The last theory which the Report before us enters on the discussion of is, that propounded by Dr. Snow, which gives a new form to the doctrine of contagion, and makes the solid and liquid *ingesta*, instead of the air, the vehicles by which the contagion is imparted to the human system. Dr. Snow's theory supposes that the poison being swallowed acts directly on the mucous membrane of the intestines, and excites the flux from its surface; that the poisonous matter is at the same time reproduced in the intestinal canal, and passes out much increased with the discharges; and lastly, that it then, in various ways, but chiefly by those discharges becoming mixed with the drinking water in rivers or wells, reaches the alimentary canals of other persons, in whom the like disease and accompanying reproduction of the poison ensue. The arguments advanced by Dr. Snow are those based on the contagion of the disease; its pathological features, which establish the drain from the mucous membrane as the source of at least the majority of the characteristic morbid phenomena; the probability that in the dark and dirty dwellings of the poor, portions of the discharge are conveyed into the stomach with the food; the appearance of cholera in those who have recently eaten food brought from infected houses, or have handled the soiled bedding of cholera patients; the extension of the disease among the inhabitants of groups of houses which were supplied from water in which the matter of cesspools, &c., was commingled; and lastly, the greater prevalence of the

disease in towns where the water used by the inhabitants was rendered foul by the contents of sewers. We shall not follow Dr. Baly in his able exposition of the value of these arguments. It is sufficient to state, that the pathological characters of the disease are explicable on other grounds; that while the communications received by the Cholera Committee do not confirm the belief that the disease is imparted, by means of the clothes and bedding of the sick, to persons handling or washing them, other facts, though apparently corroborating such an opinion, fall far short of being themselves proofs of the poisonous nature of the discharges, and render it probable that the association of a contaminated state of the water, in certain instances of outbreaks of cholera, was a mere coincidence. Many facts prove that the disease can spread through towns without the aid of contaminated water; as also that, in most instances, the epidemic has ascended rivers, instead of following the course of the water downwards; while, added to these, the relative mortality of the sexes, and other proofs derivable from the history of the disease, are together ample grounds for establishing the insufficiency of the theory proposed by Dr. Snow.

From, then, the various facts and arguments set forward in this Report, and the evidence of the other authors we have quoted, the conclusions arrived at by Dr. Baly must be regarded as deserving of our fullest approval. It has been shown, the theory that assumes the sole cause of the disease is a general state of the atmosphere, a general "atmospheric influence," or "epidemic constitution," is untenable; that the persistence of the epidemic for a certain time, even in localities of a small extent, and its very partial distribution in a country, a town, and even parts of a town, are two facts which at once suggest the inference, that the cause of the disease is a material substance, and that it is only partially distributed; that a large body of evidence renders it certain that human intercourse has, at least, a share in the propagation of the disease, and that it, under some circumstances, is the most important, if not the sole means of effecting its diffusion; while the atmospheric currents appear to share with human intercourse the office of disseminating cholera by the diffusion of the disease over limited areas; its transmission from some spots to others near at hand, rather than its conveyance to distant places which is probably effected, in the majority of cases, by the locomotion of men; that the propagation of the disease by human intercourse does not prove its contagious nature. The question, whether the poison enters the body through the lungs or through the alimentary canal, has not been conclusively solved; but no sufficient reasons

have been found for adopting the theory, that the poison is swallowed with the food or drink, is reproduced in the alimentary canal, and, being discharged with the secretions of the stomach and intestinal canal, propagates the disease, by finding access in the same vehicles to the stomachs of other persons. And lastly, that theory alone is supported by a large amount of evidence, which regards the cause of cholera as a matter increasing by some process, whether chemical or organic, in impure or damp air, and assumes that, although of course diffused with the air, it is also distributed and propagated by means of human intercourse.

Having thus closely followed Dr. Baly in the principal points which he has discussed, it remains for us to briefly notice the preventive measures he proposes. These may be classed as general sanitary rules of the most stringent character, with the inculcation of habits of strict personal cleanliness, and individual caution in dietary matters: rules which the Irish Board of Health failed not to energetically impress, and to the adoption and inculcation of which, by the various Sanitary Committees throughout our city, we have no hesitation in affirming much of our comparative freedom from the recent epidemic is mainly due. We now leave to our readers the estimate of Dr. Baly's labours, from which we have thus freely drawn, and pass to the investigation of the morbid anatomy and pathology of this disease.

The first peculiarity in reference to the post-mortem appearance of patients who have died from cholera in its cold stage—that of collapse—which Dr. Gull directed attention to is, the rise of temperature that, in many cases, has been observable on the surface of their bodies after the occurrence of death; while, where an absolute increase of temperature was not noticed, the length of time during which the body retained its warmth seemed to be, in numerous instances, remarkable, even when it might be presumed as being by no means unfavourably situated for losing its warmth quickly, by reason of the temperature at the time being moderately low, the wind coming from the east, and the window of the room, in which the body lay lightly covered, remaining open during the day-time. The muscular contractions presented by the body after death from cholera, which all who have experience of this disease must be familiar with, are next noticed. The muscles so affected being those principally of the extremities, while occasionally contractions are observed in all the other voluntary muscles, which vary in extent, from flickering and tremulous undulations in a

few fibres, scarcely to be seen or felt, to contractions sufficiently powerful to move the limbs from their position, or even to displace the body itself. They have been most commonly observed in those who died rapidly of the disease, in the middle period of life, when the muscular system was vigorous and well developed, which may, perhaps, account for their greater frequency in males than in females. When not occurring spontaneously these contractions could still be excited by percussion, or mechanical stimulus of the fibres. Dr. Gull directs attention to the fact that such are not peculiar to cholera; in confirmation of which he quotes Mr. Barlow, who observed similar phenomena in a case of severe apoplexy, fatal after six hours; Dr. Dowler, who readily produced contractions by percussion after death from yellow fever and some other diseases; and the observations of Reinhardt and Leubuscher, who had often seen contractions of individual muscles in patients dying of various diseases. Whether, or how far, these contractions depend on derangement of the force of the spinal cord, consequent on a morbid quality of the blood, as suggested by Mr. Grainger in explanation of muscular twitchings observed during the several epidemics, is a question not without pathological importance and interest. Cadaveric rigidity, it is stated, has often supervened very quickly; in one case it began at the end of an hour after death. Briquet and Mignot have observed it after forty minutes, and in most cases before two hours had elapsed. Its occurrence was not retarded by the high temperature which the body retained,—in one case being very marked, whilst the temperature in the axilla was as high as 100·58°. Rigidity occurred not only at an early period, but lasted from twenty to forty hours. Cruveilhier observes, that putrefactive changes are slow in patients dying from this disease, as in all cases where much blood has been lost, and adds, "in the alimentary canal, on the contrary, decomposition was rapid, as commonly occurs where the digestive organs are the seat of considerable sanguineous congestion." Dr. Gull, while confirming the former part of this statement, observes, that with respect to the intestinal canal the conditions were variable, and often the contrary of that noted by this great authority. The intestines containing little more than pure water, and being entirely devoid of fœcal and ammoniacal contents, were certainly not prone, in most cases, to undergo rapid decomposition.

The morbid appearances in cholera when death occured during collapse are next, by Dr. Gull, set forward with much minuteness, and the authority of eminent pathologists freely quoted. We shall in our analysis of this part of our subject include the

valuable paper of Drs. Reinhardt and Leubuscher on its morbid anatomy, a translation of which is embodied in Dr. Barwell's excellent practical observations on this disease, as also the pathological observations of Mr. O'B. Mahony. From these we learn, that, in the majority of cases, the stomach was pale and generally more or less distended. It contained turbid, mucoid fluid, grey or colourless, or tinged of a chocolate or reddish brown hue, by admixture with blood. The surface of the mucous membrane was covered with tenacious mucus, having, in some cases, a puriform character from the large admixture of exfoliated epithelium. In twelve out of thirty cases it was pale; in others there was hyperemia in different degrees, usually most marked at the greater cul-de-sac and along the greater curvature, either in a continuous arborescent form, from fulness of the smaller veins, or in irregular patches of punctate redness, occupying the summits of the ridge, and often accompanied by spots of ecchymosis. The membrane was generally rather thickened and opaque, the texture firm, and the surface mammillated. Dr. Mahony, in reference to this mammillated appearance, observes:—

"This particular state of the mucous membrane was first described by M. Louis, who considers it as a result of chronic inflammation, in which opinion M. Andral and others concur. It has, however, been found in cases, where during the lifetime of the patient no symptom of inflammation was exhibited; and the fact that this condition of the membrane is found in the autopsy of those who die in the collapsed stage of cholera, shows that it may come on very quickly, and tends to disprove that this 'mammillated' state of the mucous membrane entirely depends on chronic inflammation."

In one instance, in which death occurred thirty-six hours after the onset of the acute symptoms, slight traces of granular exudation (diphtherite) were observed extending along the greater curvature towards the pylorus. In four cases the solitary glands are stated to have been enlarged; in two they were seen near the cardia and termination of the œsophagus; and in two near the pylorus. At this part also the membrane was occasionally dotted over with small pits, which Dr. Gull states were probably produced by the bursting of these glands from serous effusion into their cavities, a result which has frequently been observed in the solitary glands of the small intestines. These observations are partially or wholly confirmed by other authorities quoted. Dr. Gull, though having insufficient data to form an opinion respecting the condition of the pharynx and œsophagus, at the same time mentions that in one

case, fatal after twelve hours of acute symptoms, it was observed that the lower part of the œsophagus was deprived of its epithelium. The state of these parts is described by Reinhardt and Leubuscher, in Dr. Barwell's work, as on three occasions presenting traces of diphtherous deposit. Other observers have noted the same in a less degree.

In many of the reports furnished to Dr. Gull, no note is made of the peritoneal surface; in some it is stated to have been rather dryer than natural, and covered with a slight layer of a tenacious mucoid secretion. Briquet and Mignot, in their report, state that,

"The peritoneum was carefully examined in thirty-two cases. In eleven there was universal and intense injection of the small capillary veins of the sub-epithelium cellular tissue; in the other cases the same condition existed, but in a less degree. It was in general most marked on the depending surface of the intestines, and on the organs contained in the pelvis, and especially on the genital organs of the female."

In no instance have fibrinous exudations in the cavity of the peritoneum been found by these authors. The condition of the small intestines has received the closest attention; their walls were thickened and pulpy from œdema of the mucous membrane and submucous tissue. The duodenum and ilium were more commonly affected, and to a greater extent than the jejunum. In some instances the mucous membrane was pale throughout, in others the lower part of the ilium only, or the duodenum only was hyperemic. The vascularity of this membrane was observed either as a uniform arborescent venous injection, affecting large tracts of the intestine, particularly the lower part of the ilium; or as patches of variable extent in which the redness was punctate, and of a bright colour, frequently with spots of ecchymosis and an exudation of tenacious bloody mucus. The villi were also swollen and prominent from œdema, especially throughout the jejunum. Where the morbid process was most intense as regards the mucous membrane, upon its surface and within its tissue was sometimes observed an effusion of a yellowish finely-granular fibrinous matter, such as occurs in ordinary diphtherite. The glandular structures were the seat of changes similar to those observed in the mucous membrane itself. Dr. Gairdner, in his report on the morbid appearances in eighty-nine cases of cholera, says,

"The most frequent of all the abnormal conditions of the mucous membrane, was the prominence of the intestinal glands, both

of the aggregated and solitary, but especially of the latter. This condition, the 'psorenterie' of some French writers, was found in about two-thirds of the cases."

The same condition extended to the patches of Peyer. The mesenteric glands generally presented no morbid changes; the large was more rarely affected with hyperemia and ecchymosis than the small intestine. In many instances it presented nothing abnormal beyond a greater distinctness of the solitary glands.

The general appearance of the evacuations in cholera is well known; their reaction was alkaline or neutral. Dr. Parkes has particularly noted the small amount of the so-called extractives, or incoagulable organic substances, thrown out in cholera, a circumstance which seems to indicate a suspension of the proper excreting function of the mucous membrane during the algide stage. This observation is considered important as bearing upon the pathology of the disease. The observations of various pathologists respecting the appearances presented by the spleen, may be expressed in the opinion of Virchow, who, in a report of seventy cases, states, that he arrived at no definite result as to the morbid changes in that organ. The condition of the liver after death, in the algide stage of cholera, may be set down as being generally diminished in bulk, the tissue flaccid, and the capsule finely wrinkled. The larger veins, both the hepatic and the portal, but especially the latter, were often full of dark, viscid blood. The lobular appearance of the secreting surface was indistinct, and the whole tissue of a rather lighter red than usual. The secreting structure, on a microscopical examination, presented nothing abnormal. The gall-bladder was usually distended with dark bile, generally viscid, but sometimes more watery than natural. In most cases the gall-ducts were not obviously affected. The mucous membrane of the gall-bladder and ducts was healthy, except in some rare cases, where it was the seat of morbid changes, similar to those occurring in the intestinal mucous membrane. The kidneys were of the natural size, their surface was mottled by arborescent venous injection, and, on section, the same venous hyperemia gave a dark colour to the cones. The secreting structure rarely presented any obvious morbid change. As regards the thoracic viscera it was observed that, in the majority of cases, fatal in the algide stage, no other morbid change existed than engorgement of the lower and posterior parts of the lungs with dark blood. The heart and pericardium usually presented little that was abnormal. Reinhardt and Leubuscher remark, that the right auricle and

ventricle were generally distended with blood, the left containing, in proportion, but a small quantity.

Most observers admit that the physical condition of the blood in the cavities of the heart and large vessels was more commonly dark and fluid, or less coagulable than in other diseases. Dr. Gairdner differs from this opinion, and believes that the epithets "dark" and "venous" are, in no degree, more applicable to cholera blood *after death* than to that in every ordinary form of fatal disease. Notwithstanding this excellent authority, the weight of evidence tends to prove that, in a large number of cases, the coagulable properties of the blood were lessened, while the blood drawn during life, in the cold stage, was notoriously viscid and tar-like,—a change, as will be seen, depending upon a loss of its water, and imperfect oxidation arising out of venous retardation. With respect to the alteration in the saline constituents of the blood, Dr. Gull quotes the investigations of Dr. Carl Schmidt and Dr. Garrod. We transcribe the conclusions arrived at by the latter:—

"That in cholera the saline constituents of the blood are not only not decreased in amount, but sometimes exist even in increased proportion, and that the diminution of its alkaline reaction is not due to the loss of salts, but to the impeded excretion of organic acids, which are constantly being formed in the system.

"That urea usually exists in increased quantities in cholera blood, but that the amount differs considerably in the different stages of the disease; but being small in quantity in the intense stage of collapse, increasing during reaction, and in excess when consecutive febrile symptoms occur."

The morbid appearances, when death occurred after reaction, may be generally stated as maintaining the characteristics we have just mentioned, though generally in a less prominent degree.

The *pathology* of cholera, as deducible from the foregoing cadaveric phenomena, must lead us to regard the gastro-intestinal mucous membrane, with its ganglionic nervous centres, as the focus of the morbid action. Whether there be a primary absorption of poison into the blood or not is at present unknown, though our knowledge of the laws by which many deleterious agents produce their effects renders the former hypothesis highly probable, namely, that a specific poison acts upon the ganglionic nervous centres, or upon the mucous membrane itself. The examination of the fluids effused from the mucous membrane gives no evidence of active plasmatic changes taking place in them, on the contrary, the large amount

of fluid thrown out, its low specific gravity, and its other physical characters, indicate an almost passive exosmosis as through a dead membrane. Some observers have referred the morbid changes to a *catarrhal condition*, others have regarded the disease as a form of *serous hemorrhage*, and the Berlin pathologists have designated it *a destructive diphtheritic inflammation.*

Dr. Gull well writes:—

"We believe that, for the present, such generalizations, however plausible, are of little value, and that we arrest inquiry by their adoption. The depression of the capillary power, the extreme exhaustion of the great ganglionic nervous centres in the abdomen, the passive character of the lesions of the mucous membrane, its normal action being reversed to a fatal exosmosis, are peculiar to cholera, and give it an individuality which forbids our merging it for the present in any general category."

There is an hypothesis regarding the nature of cholera, based upon the supposition of a suppression of the hepatic secretion, and consequent congestion of the liver. This hypothesis is unsupported by anatomical facts. The hepatic function does not appear to be subject to any further derangement than that which naturally *follows upon* the retardation of the circulation during the stage of collapse. This is an important fact; for erroneous suppositions on this head have widely influenced the treatment of the disease on the part of those who, with Dr. Ayre, advocate the administration of calomel. This gentleman writes:—

"Now there is one condition which is uniformly and conspicuously present in malignant cholera, and is, indeed, characteristic of it, namely, a suppressed or suspended secretion of bile, as shown by the diminution, and, at length, the total disappearance of it from those watery discharges which are poured so profusely from the stomach and bowels. As a consequence of this cessation of the hepatic function, an accumulation," he adds, "will take place in the liver, of venous blood, and an impeded circulation result from it, producing a congested state of that organ; and, subsequently, by a retention of the blood in its course through them, of those abdominal organs whose circulation is associated with it. Now, the congestion thus produced in the portal venous system of the liver and its associated organs constitutes the stage of collapse, and under various modifications and grades of intensity, whose real nature and amount are unknown, forms the essence of it in all."

Such a train of reasoning, as Dr. Gull observes, "is unsupported by any evidence." The serous, rice-water character of the cholera stools is obviously due to special pathological changes in the mucous membrane, and not to any merely me-

chanical congestion of a secondary kind, as here stated. The appearances after death in the chest and cranium show that the viscera in these cavities were not primarily affected. The same observation applies to the congestion of the veins, and the effusions under the membranes and into the ventricles of the brain. Although the blueness of the skin, in many cases, may be in part due to a general retardation of the venous circulation, it is, we believe, more commonly owing to a condition independent of such a cause and appears to be a pathological indication of a loss of tonicity in the capillaries and capillary veins. Many of the symptoms of the cold stage evidently depend upon the loss of fluids from the blood, and, consequently, have been removed in a sudden and remarkable manner in those cases where saline injections have been successfully thrown into the veins.

Having so far detailed the pathological appearances and inferences, which we have but briefly analyzed, Dr. Gull proceeds to inquire—Is cholera a zymotic or contagious disease? In reference to the former, he observes:—

"Cholera has been classed amongst zymotic diseases; but its clinical history and morbid anatomy are opposed to the theory of its being due to zymosis, in a strict sense of the term. In a zymotic disease, the ζυμη induces certain plasmatic changes, from which results its augmentation. In cholera we have no evidence of such changes, the alterations in the blood, as far as they are yet known, being referable to the loss of its fluid parts, in accordance with the physical laws of exosmosis. The local morbid action appears to be of a negative rather than of a positive kind. The marked depression of the organic functions, and the morphological characters of the effused fluids, as well as their general physical properties, indicate a passiveness almost peculiar to cholera. In cholera the onset of an attack is frequently sudden, and the effects apparently direct, such as would follow the immediate action of an extraneous poison upon the body. The zymotic process is more gradual, and the symptoms follow a more constant rule with respect to time. Cholera appears to consist of but one single series of actions, which may vary in intensity through every gradation, but, throughout, maintain the same character of passiveness. There is no febrile stage to which zymotic changes, as ordinarily observed, and the production of a materies morbi, may be referred."

The theory of Dr. Snow, of the direct application of a materies morbi, is at variance with the remark of Schmidt, that those who were occupied in examining the discharges, and inhaled the effluvia of them, felt no ill effects. These discharges were also brought into contact with abraded surfaces with im-

punity. They who were engaged in making post-mortem examinations of cholera subjects seemed to incur no risk of thereby taking the disease. Schmidt states, that within his own knowledge, a drunken man by mistake swallowed half a beer glass of the vomited matter, slept away his drunken fit, and remained well. He adds that many similar facts were made known during the epidemic of 1831-32, when medical men, by way of experiment, swallowed these transudations without injury; but, with laudable humility observes:—"I have not had self-denial enough to institute these experiments on myself." Mr. Marshall remarks that the performances, here alluded to, were limited to *tasting* the *vomited matter* only, and did not extend to *swallowing* the *alvine evacuations*. On this we shall only observe *chacun à son gout!*

The report proceeds to observe that many experiments have been made upon animals with the blood and effused fluids in cholera, with the view of determining whether the disease is communicable by them. Fresh cholera blood and filtered rice-water fluid have, in different instances, been injected into the veins of dogs, cats, and rabbits, without producing any effects that indicate a specific poison, the results being only a temporary depression, and in some cases slight diarrhœa. The evacuations have also been thrown into the stomachs of these animals by Mayer and Marshall, but with uncertain results. In eleven instances no ill effects followed, beyond temporary depression and slight diarrhœa. In six, death took place in from twenty-four hours to five days. The experiments instituted by Dr. Lindsay, and detailed in a pamphlet reprinted from our esteemed cotemporary, "The Edinburgh Medical and Surgical Journal," furnish results somewhat different. To its pages we shall refer our readers as setting forward the climax of man's invention for experimental research, and the acme of animal suffering in its ministration to science. These experiments lead to the inference, that cholera was simultaneously developed in four animals, and proved fatal in two of them. Mr. Marshall observes of his experiment, that,—

"Equivocal rice-water discharges, blueness, coldness, cramps, tarry blood, and non secretion of urine, are not in the catalogue of the effects, and so long as we know so little as a ground of comparison of the pathology of natural cholera in animals, we cannot draw safe conclusions from the phenomena produced by the administration of the cholera evacuations."

Though Dr. Lindsay's experiments seem to argue more directly in favour of the communicability of the disease, we

cannot divest our minds of the supposition that, had equally miserable animals been subjected to a similar ordeal, from which cholera infection might be presumed as being excluded, closely analogous results would equally have ensued.

The next subject of importance, entered on in the report, is the consideration of the *premonitory diarrhœa*, its characters and frequency. This has been made the ground of a special essay by Dr. M'Loughlin. The numerous replies received to the queries of the College of Physicians, all serve to establish the *frequency* of a stage of diarrhœa, lasting from a few hours to several days. Dr. M'Loughlin, in wrathful indignation at such a statement, leads us to believe that, without diarrhœa there can be no cholera, and having eased his mind in this particular by a summary detail of cases, informs us, that the College of Physicians have not in their Report advanced our knowledge of cholera on any one single point,—an affirmation which falls harshly on our ears, considering we have spent not a few hours in carefully studying its contents, as well as in submitting them to our readers. We trust, however, for the sake of science, this very candid expression of individual opinion will not occasion the labours of Dr. Baly and Gull to be considered as altogether valueless, or wholly prevent the diffusion of their work. In reference to the inquiry as to whether the premonitory diarrhœa of cholera is characteristic, we are informed that the diarrhœa premonitory of the severer symptoms of cholera was often feculent and bilious, and presented no characteristics whereby it could be certainly distinguished from other forms. There were, however, occasionally some subordinate points of slight diagnostic value; the evacuations were generally more profuse and liquid than usual, but otherwise of a natural appearance, often unaccompanied by pain, and passed without effort, the painlessness and passiveness giving a false security to the patient. It was not until the nervous system began to be depressed, and the feculent character of the stools was lessened or lost, and they became alkaline, watery, and flocculent, that they were distinctive. On this point, the experience of the profession appears to be uniform, and hence we may draw the following inportant conclusion:—that during the prevalence of the epidemic every case of diarrhœa, arising without obvious cause, may be regarded as a probable result of the specific poison. The question as to how far collapse depends upon loss of fluids is next discussed. Dr. Gull observes, as many of the symptoms of this state of collapse depend upon the loss of fluid, it has been too absolutely inferred that the general phenomena of the disease are always in a necessary re-

lation to the amount of these effusions. In tropical regions, where either the intensity of the poison is greater, or the predisposing conditions of constitution are more favourable to its operation, it appears by no means unfrequent for the strongest subjects to fall into sudden collapse, without any very notable loss of fluid. Such facts are authenticated by so many careful writers, as to leave us no doubt of their occurrence. In temperate regions, similar cases, though rare, are not unknown; and in a less marked degree, are within the experience of most who have seen the disease in its severer form. Dr. Gull adds, even when the loss of fluid is very great, it is doubtful whether death is due to it alone, since we often see patients in an apparently equally hopeless state, collapsed, and bloodless, whose tissues, as soon as the nervous system begins to react, recover their elasticity before any amount of absorption could, from the circumstances of the case, have occurred.

Next in the Report follows the pathology of the stage of reaction. The so-called consecutive fever has been divided into different forms according to the prominence of particular symptoms. Such divisions we must regard as being of little practical value, as their boundaries are not constant, and they mislead by fixing the attention exclusively upon one organ, where, from the circumstances of the case, the whole system is more or less deranged. Amongst the most important of these secondary lesions those of the kidney are foremost. Not only are these organs, in the first stage, occasionally subject to the cholera process, but, from the complexity of their circulation, the state of the blood, and the depressed nervous power of the ganglionic system, they slowly recover their function, and, from the persistent congestion, the secreting structures undergo changes similar to those found in acute albuminuria from other causes. As to the frequency of albumen in the urine, after the cold stage of cholera, there is a universal accordance amongst different observers, both in this country and on the Continent. The communications received by the College show, that the symptoms of the consecutive fever were most frequently referable to the defects in the urinary secretion. The frequency of the occurrence of this fever varies according to the experience of each. The mortality from the disease generally leads to the inference, that nine-tenths of the deaths took place in collapse, or during imperfect reaction, and not more than one-tenth in the consecutive fever, which is stated as being probably above the average. The occurrence of a cholera exanthema, as first noticed by Dr. Babington, is stated as not being of, by any means, frequent occurrence. The sta-

tistics of deaths from cholera prove that the epidemic was especially fatal in early life and old age, while the observations of Briquet and Mignot affirm, that constitutional weakness alone was not a very evident predisposing cause to this disease. Difference of sex does not appear to have exercised any material influence on cholera. Respecting the result of habits, Mr. Grainger's observations may be received, that "abundant evidence was afforded during the late epidemic, that habitual drunkards were highly predisposed to cholera, and of them a large number perished." This section of the Report concludes by an investigation of the relation of sporadic to epidemic cholera, on which subject we subjoin the substance of Dr. Gull's observations:—

"Our knowledge of the morbid anatomy of sporadic or English cholera is too defective to enable us to institute a strict comparison on this head between it and the epidemic form; nevertheless, facts are sufficient to show that there is an essential difference between the two diseases."

It will be evident, from the foregoing observations, that the state of our knowledge of this disease rests equally on negative as positive data,—who will take upon themselves to declare which is of the greatest value? Those who, uninfluenced by theory, dispassionately reflect on the information we have supplied, will, we are confident, freely admit, that in satisfying us of what cholera does not indicate, and in showing us those physical changes which more particularly accompany the state of collapse, pathology has not failed, in the hands of Dr. Gull, to render good service to the cause and progress of our science; while, at the same time, they must, to use the words of Dr. Cormack, avow with frankness their conviction—

"That, notwithstanding all that has been written regarding cholera, sufficient data do not yet exist to entitle any one to feel quite confident that their opinions are built upon an unassailable foundation."

This physician's admirable little work will be found to contain a chapter on the manner of studying cholera, which merits our warmest approval; from which, previous to proceeding to the consideration of the treatment of this disease, we extract the following passage:—

"Therapeutical experiments must be jealously corrected by the observation of simple cases, which have been mainly confided to nature; and *recoveries* must not be promiscuously set down as *cures*."

For one who has carefully reflected on the probable etiology and pathology of this disease, fifty have proposed specific remedies. During the late epidemic, seldom a week passed without the public being informed of some undoubted cure newly sought out. Ignorance and folly painfully conjoined to trifle with human life; and, with the best possible intentions, caused a considerable deal of mischief. We can understand how a number of kind-hearted, soft-headed, good-natured, worthy old folk, hearing that something did a great deal of good to somebody, should at once fancy that it would be not only useful, but right, "to make that public," and in an outbreak of philanthropic enthusiasm, lose no time in "writing to the papers," and then, perhaps, think no more about it; but how individuals pretending to medical knowledge could pen half the sheer nonsense that has appeared on this disease we know not. The most dangerous writers of the latter class are those who, confounding symptoms with the disease they indicate, and mistaking consequences for causes, originate or adopt some erroneous pathology, which they henceforth religiously believe in, and very perseveringly adhere to. The strongest feature appertaining to this medley of opinions is the extraordinary success said to be attendant on the employment of the means which each one advocates. He who stimulates, and he who depresses; he who bleeds, and he who does not; he who gives aperients, and he who abhors them; he who administers mercury, and he who withholds it,—in fact, they who run diametrically counter one to the other, are ready to produce the most convincing proofs of astonishing cures when everything else had failed.

That cholera is a fearful and terribly fatal disease is, alas! too evident; that in many cases our best exertions fail to arrest the hand of the destroyer is, we confess, equally true. Are we, therefore, as medical philosophers, to succumb to the general panic? Is it fitting that we abandon the solemn trust committed to us, and, grasping the shadow of boasted specifics, lay aside those several remedies which have done us good service in the hour of trial? We have had very painful experience of this affection; and though in a large number of patients under our charge, a considerable, but by no means unusual, mortality ensued, we have still felt satisfied, that for their aid medicine had not been wanting, because disease had triumphed. The truth is, that in very many cases the best directed human means must prove unavailing in this, as they do in other affections, in which the vital powers receive a shock sufficient to paralyze the several organs through which remedies can alone act,—when

a condensation of disease, as it were, takes the citadel by storm, while, at the same time, intercepting the supplies for its relief. The danger we would, with all earnestness, warn medical men against is, the losing confidence in their medical resources, and adopting popular remedies. The first is evidence of a weak, vacillating mind, that dares not look danger in the face; the second betokens a craven spirit, which hopes that its own errors may either pass unnoticed among, or be eclipsed by, those of others.

Our etiology and pathology of this disease have shown when and how our efforts should be directed. It is, as the report informs us, well ascertained, that in the largest proportion of cases, at least in European countries, the poison of cholera produces its first effects on the system *gradually*, as indicated by diarrhœa, varying in duration from a few hours to several days before intense symptoms supervene. During this, the period of invasion, it is highly probable, nay, experience assures that it is certain, the morbid effects may be at such a stage *often* successfully combated. The necessity of correctly estimating the significance of this premonitory symptom cannot be too fully impressed. The Irish Board of Health, whatever the peculiarities of their views may have been, did good service to the community, when they wrote:—"Let it then be clearly understood, that when the epidemic is prevalent, mere looseness of the bowels, with or without pain, may be the commencement or first stage of cholera; that the disease is generally curable in this stage; and that not a moment should be lost in applying for relief." Some have been found, who, regarding this symptom as an effort of nature to throw off a *materies morbi*, have applied their theory to practice, and thereby increased the fatality of the disease; while others, adopting equally erroneous pathological views, have advocated specific remedies, which they have regarded as curative agents, because under their use the patient recovered.

We accord to the sentiments of the Report of the College of Physicians, that no antidote or specific medicine is known to neutralize the cause of cholera, or with certainty to arrest its effects; from this we have then every right to infer that the closest adherence to those medical principles which the analogy of various diseases, and a just estimate of post-mortem appearances furnish, argues most favourably for the general result, when, to use the words of Dr. Cormack, "a more successful treatment is to be discovered by a judicious application of the means which we possess, than by searching for some new specific." Though the amount of success obtained by early treatment could not be

accurately determined, the general results of preventive measures must be regarded as being apparently very favourable, as shown by the small proportion of cases which passed into the severer forms of the disease subsequently to early treatment.

The first matter for the consideration of the practitioner is the position most suitable for an individual attacked. A recumbent position is, as the Report impresses, proved by experience, and also by the nature of the case, to be a most important measure. It prevents exhaustion, favours the circulation, and lessens the frequency of the evacuations. It is highly probable that cases, which otherwise resisted the action of medicines, would have readily yielded had the horizontal position in a warm bed been strictly enforced.

In the treatment of the premonitory diarrhœa, it cannot be a matter of doubt that the earlier the disease is encountered, the greater, in an infinitely high ratio, are the advantages under which medicines are employed to counteract it. What those medicines are, which experience proves to be most deserving confidence, is a question that must receive a general rather than a special reply. We would state them to be—first, those known to exercise a certain power over the depressed energies of the affected intestinal mucous surface; and second, those calculated to sustain the diminished and impaired vitality. Many years since Dr. Graves advocated the employment of acetate of lead and opium, in combination, as pills, to be given every half hour until the rice-water discharges from the stomach and rectum began to diminish. Since then many writers have borne testimony to the value of their administration. It is needless, therefore, we add to theirs our own experience in their favour, further than to say that, in a fluid form, the same remedies have at times seemed to better fulfil that which was required. The value of this combination seems to rest in its proving an astringent and sedative of great power, which exercises a beneficial influence in checking the serous discharges, and thus, in a measure, preventing one of the chief elements in the production of that collapse so generally fatal. Various remedies of this class, administered with a similar intention, have been advocated by different medical men, and each has borne testimony to their particular advantages, not as specific remedies, but as means generally conducive to promote the same end. Amongst these we might mention many tonics and astringents, both mineral and vegetable, as well as other remedies known to exercise certain actions. Thus, nitrate of silver, sulphuric acid, nitrous acid, nitro-sulphuric acid, alum, gallic acid, quina, creasote, and turpentine, have each, under particular circumstances, been

employed with advantage during the approach of this disease. In the fully-developed stage it is obvious that medicines administered internally must be of small power, since the pathological condition of the gastro-intestinal membrane is such that absorption is then almost, if not quite, suspended; and medicines when retained in the stomach form but an inert accumulation. Dr. Gull's observation on their administration at this period we particularize:—

"The theory of their occasionally acting by sympathy, which must be regarded as a relic of a therapeutic superstition long since exploded, and untenable in any condition of the system, is especially so in the collapse of cholera."

The calomel treatment of cholera stands foremost, from having in this country been more fully tested than any other. The theory of the disease, which has chiefly led to its employment, is not supported by anatomical facts. The absence of bile from the evacuations appears to be merely a subordinate result. Calomel can be administered only on empirical grounds, and its value must be determined by the results so obtained; for there appears to be no argument in favour of its exhibition either from analogy or pathology. How far Dr. Graves had anticipated these sentiments of the Report may be inferred from the following quotation expressive of his opinions in the year 1832:—

"Before we proceed further, I may observe that the principle on which the calomel treatment was employed in cholera arose from almost constantly observing that there was a total deficiency of bile in the stools. Soon after the supervention of an attack, the alvine discharges were observed to be white, and without the slightest tinge of bile; and on this very remarkable symptom practitioners dwelt almost exclusively, thinking that the patient's only chance lay in restoring the secretion of the liver. Now it is obvious that the absence of bile in the stools is no more a cause of the disease than is the deficiency of urea in the kidneys, or of serum in the blood. Viewing the disease in this light, it would be just as reasonable to give a diuretic to restore the secretion of the kidneys, as to give calomel to produce a flow of bile. The liver ceases to secrete, not only in consequence of the injury done to its vitality by the proximate cause of cholera, whatever that may be, but also from a mechanical cause, namely, from a diminution in its supply of blood."

Notwithstanding these, which we consider sound pathological views, very many practitioners adduce extraordinary testimony to the efficacy of calomel as a curative agent. Dr. Ayre particularly, in a pamphlet already referred to, affirms that calo-

mel, in small doses, according to prescribed conditions, and without any other adjuvant than cold water *ad libitum*, exhibits a remedial power well nigh approaching those of a specific, and very loudly cries out against Dr. Gull's conclusions, that calomel was inert when administered in collapse, and that the cases of recovery following its employment at this period were due to the natural course of the disease, as they did not surpass the ordinary average obtained when the treatment consisted in the use of cold water only. We do not question the truth of Dr. Ayre's assertion, that many recovered while taking calomel according to his method of prescribing it. Recoveries are not, however, cures. We fully agree with Dr. Gull, that an analysis of Dr. Ayre's cases does not sustain the favourable opinion he would impress respecting this remedy, since the deaths were 365 out of 725 unequivocal cases; while, under various and opposite plans, the recoveries, even in severe cases, averaged from 45 to 55 per cent., according to the period of the epidemic. That Dr. Ayre's views of the action of this remedy are based on erroneous pathological conceptions we fully believe.

Treatment by calomel, opium, and stimulants, or that which has been termed the "rational treatment," has not proved much more satisfactory. The combination of these three remedies promised to fulfil the supposed indications of this stage. The calomel was given to restore the functions of the liver, and as an alterative of the morbid action in the gastro-intestinal mucous membrane; the opium to allay irritation and arrest the discharges; and the stimulants to counteract the depression of the nervous system. Experience has not responded to the expectation such a combination aroused. At the same time the results were not altogether so indifferent as when calomel was exhibited by itself. Although opium and diffusible stimulants, brandy, camphor, and ammonia, were useful at an early stage of the disease, as collapse set in they not only failed to produce any favourable result, but were after found to aggravate the symptoms. Stimulants, especially the various preparations of alcohol, did not act as restoratives in collapse, but often increased the irritability of the stomach, and added to the sense of oppression at the præcordia. On this subject Dr. Haines, in his pamphlet, containing many practical and valuable remarks, thus writes:—

"Circumstances which at an early period came to my knowledge, together with observation and reflection on the progress of the disease, induced me soon to discard much of the stimulating plan of treatment; and in the epidemic of 1834 to never once recur to the use of brandy or other strong spirituous liquors."

The evidence before the College of Physicians of London is sufficient to prove that the results of practice were very much in favour of that method of treatment and employment of remedies which Dr. Graves may be said to have been the first advocate of. Opium, in the stage of collapse, was found injurious, as increasing the cerebral oppression, and embarrassing the system during reaction. The experience of the profession, as conveyed to Dr. Gull, is evidently at variance with the use of stimulants, which are generally described as tending to aggravate the disease. Chloroform, of which much was expected, has been found evanescent in its effects and unsatisfactory in its results. The use of cold water and ice has, in the majority of cases, been attended with at least a mitigation of the suffering, and proved at the same time grateful, urgent thirst being a prominent symptom. Cold water was the drink generally preferred, and good results were often observed when it was taken freely, in repeated and copious draughts, although it excited vomiting. In smaller quantities, and iced, it was refreshing to the system, and allayed the irritability of the stomach. Ice was generally grateful to patients in impending or approaching collapse, and probably acted favourably upon the mucous membrane, and served to arrest the discharges.

The use of salines has been energetically adopted, and warmly advocated by many who, with Dr. Stevens, believed that their administration tended to restore to the blood a fluid similar to that lost in the early stages of the disease. There is no evidence that they possessed any influence over the local morbid action in the mucous membrane. It was not until this surface had in part recovered its function of absorption that any good resulted from their employment. The use of emetics originated also from erroneous views of pathology. They were given, at the onset of the disease, with the intention of cutting short the morbid action, by distributing the blood to the surface, and relieving the congestion of the intestinal mucous membrane; and they were also given for the purpose of superseding the irritability of the stomach set up by the disease. Their administration generally failed to produce the anticipated results.

Dr. Reid[a], whose lengthened experience and deserved reputation render all his observations worthy of close attention,

[a] *The Pathology and Treatment of Cholera Asphyxia.* By ROBERT REID, M. D. T. C. D., F. K. & Q. C. P. I. Second Edition. Dublin: Hodges and Smith. 1855. Pamphlet, pp. 29. We regret that we did not receive this practical little essay until the greater portion of our review had been printed off, as we are thus precluded from noticing it at length.

has in the pamphlet before us described a condition of the system during cholera epidemics, in which the natural secretions are evacuated in increased quantity. In these cases he has remarked that a mustard emetic, followed by a warm purgative, has relieved the patient entirely of his disease, and, as he believes, has prevented the access of more serious symptoms. The selection of cases suitable for such a treatment would, we conceive, involve a large amount of practical knowledge.

Bleeding was formerly and is still by some advocated; experience, however, leaves no doubt that, in the premonitory and early stages, it is in general to be avoided. In the consecutive fever, local depletion has seemed to be more directly indicated; in this condition experience also shows that its employment requires much caution.

We might extend this notice of remedies to a much greater number, and include many which the failure of legitimate medicines has called into existence. It is foreign from our present purpose to do so. In affording our readers the assurance of the College of Physicians, which we have already stated, *that at present no antidote or specific medicine is known to neutralize the cause of cholera, or with certainty to arrest its early effects*, we bid them but the more strenuously cultivate that knowledge which is power, being satisfied that it alone can enable them to meet the disease when the struggle comes.

The treatment of the secondary fever it is unnecessary for us to enter on, since each case may be a study in itself.

There are many valuable communications in reference to the pathology and treatment of this disease which have at intervals appeared throughout the pages of our Journal: these we would gladly refer to did the necessarily limited range of such a review as the present admit of our doing so. We must, however, hasten to bring our labours to a conclusion, feeling satisfied that though we have offered no rule for guidance, yet in calmly and dispassionately discussing what and how much we know, in admitting that on many points our knowledge is but limited, and our power but slight, we afford at least sufficient materiel to the reflective mind, which we doubt not will bring forth good fruit in its due season.

Quarante Années de Pratique Chirurgicale. Par Ph.-J. Roux, Chirurgien de l'Hotel Dieu, &c. Tome I. Chirurgie Réparatrice. Paris: Masson. 1854. 8vo, pp. 474.

When a man has laboured with honour and success in his vocation for well nigh half a century, and has passed away amid the regrets and reverence of survivors, it is impossible to notice a work that bears his name in a spirit similar to that which would influence us in commenting on the labours of the living. Criticism is to a great extent silenced, or at most it speaks with warning voice to the bystanders. In such cases the best mode of fulfilling our duty to the public is to point out the position which, in our opinion, is due to the author in the page of history, and as far as possible to dissociate him from the accessories which the personal feeling of contemporaries is prone to place around him. This process, frequently disagreeable and at no time easy, enables all to judge for themselves, and to weigh opinions and statements at their legitimate value. In the present instance there is less difficulty in arriving at a proper estimate, in consequence of the great unanimity which exists among those who have been M. Roux's associates, in their view of his character. The extreme kindliness of his nature, his gentleness to his patients, and fatherly good-nature to his pupils, have endeared his memory to many hearts. Few, we suppose, who enjoyed such popularity with the public, were so free from enemies among professional rivals. There was indeed a sort of competition between him and Dupuytren during the life of the latter; but the calibre of the men was so different, and their special merits so opposite, that the contest was carried on upon unequal terms, and we believe scarcely went beyond the bounds of fair professional emulation. Roux never had the force or scientific prescience of his great rival: his merit was peculiarly that of the operative surgeon; delicacy and neatness of hand were his great characteristics; and many of our readers can no doubt recall to mind the elegance with which he used his bistoury, and his fancy for operations requiring careful dissections. The manual part of surgery was his forte, and he cared little for exercising his skill except upon it; he was no theorist, and contributed wonderfully little to the science of the profession; his writings are neat in their style, but, like his oral instructions, diffuse and deficient in logical force. His extreme *bonhommie* never allowed him to contradict any one, or to advance his opinion in a manner calculated to wound the feelings of another; hence they are sometimes expressed in terms so ambiguous as to render it a matter of difficulty to

ascertain them with exactitude. The form chosen for the work at present under review has led to an exaggeration of these faults of style; it is epistolary, the letters of this first volume being addressed to Mr. Lawrence. We are told in the editor's preface, that the composition of four volumes was complete, and that the first had been almost printed when M. Roux's death took place, and that a commission of five members of the Society of Surgery of Paris has undertaken the revision of the work.

The present volume is devoted to "Chirurgie Réparatrice," which is thus defined:—"When the hand of the surgeon reforms after any manner, or reconstitutes a part which is deficient or seems to be deficient, which is destroyed in whole or in part, or at least appears to be so: when it causes a part to spring up afresh, which appeared not to have any existence, or which in reality had none."

The tendency of his mind to give undue prominence to minutiæ rather than to seize upon broad principles, is well shown by the many pages which are occupied with arguments in favour of sutures, and remarks on the various kinds of suture adapted to different reparatory operations; the substance of which may be expressed in a few words. The twisted suture is his favorite, and in shallow wounds is most generally useful; he discards it in operation on the perineal region, having found it fail in his first case; the quilled suture succeeds better here, as in all cases where it is essential to procure union of a deep wound. In staphyloraphy the simple suture obtains his favour, but we have not found any mention made of Mr. Fergusson's use of the slip-knot or common noose, which is the easiest and quickest of application. A large space is devoted to operations upon the palate: these were his especial domain, and he asserts with just pride his claim to be the originator of Staphyloraphy. A sketch of the circumstances attending his first case may not be uninteresting to our readers. In the year 1819 a student named Stephenson, a native of Canada, presented himself to M. Roux at the conclusion of his studies in Paris; in the course of conversation M. Roux discovered that he laboured under a simple congenital fissure of the soft palate; a malformation of which he had not previously seen an example; upon the instant he conceived the idea of remedying it by operation, not being aware that Graefe had tried it three years previously in a similar case, without success. The operation was performed two days after, and Mr. Stephenson was able in eleven days more to read an account of the case at the Académie des Sciences. Ultimately the improve-

ment in his powers of distinct articulation was so great as to enable him to fill the Chair of Anatomy at Montreal. The full result of this success is given in the following terms. "I have performed suture of the velum palati for congenital fissure one hundred and twenty times; and if I add to this first great category the various other cases to which I have had to apply staphyloraphy, properly so called, or some similar operation, I attain a total of 140 cases." The great similarity of most congenital fissures of the soft palate renders the operative proceeding in general alike in all. M. Roux's routine practice consisted in, first, placing the threads; secondly, paring the edges; and thirdly, tightening the ligature; he preferred to commence by inserting the ligature, probably because he did so in his first case; his assigned reasons scarcely satisfy us. He says he never tried the other order of procedure, although he found the loops of the ligatures in the way, and liable to be cut while the edges were being pared, an accident which happened to him several times. The only case in which it seems to us advisable to follow M. Roux's inverted order, is when we fear that the patient will lose courage, or become unruly, and so compel us to desist altogether from the operation; in such a case the insertion of the threads might be first proceeded with, and if they require to be withdrawn, no appreciable injury has been done to the soft parts, and at some more favourable opportunity the operation may be again attempted with the parts in as advantageous a condition. Care in selecting the proper age will probably suffice to guard against this *contretemps*, which even in M. Roux's extensive practice only occurred twice. He invariably inserted the ligature from behind forwards; each ligature consisted of two or three threads flattened into a little ribbon, and well waxed; he used three points of suture, tying the inferior first, then the superior, and last of all the middle one. When the hard palate was cleft or perforated, he seems to have preferred a plate or obturator to remedy this portion of the malformation, and the cases which he gives as the exceptions to this practice only amount to four; in these the operative procedure was after the Indian method, and consisted in detaching a portion of the membrane covering the bones at either side, leaving it attached by a pedicle; and by a movement of rotation approximating it to its fellow on the opposite side, and there uniting it by sutures: in three of the cases he succeeded; in the fourth the flaps mortified.

There is another mode of filling this space, which has been tried with success by, we believe, Mr. Avery of London. It is founded on the observation that in these cases the bones are

not only separated, but form portions of a much higher arch than natural; hence it is found possible, by detaching the fibro-mucous covering from them extensively, so as to form flaps adherent only at the alveoli, to bring the edges of these flaps freely in contact at a level below that of the bones, especially if this dissection be continued back to the posterior margin of the palatine bones, thus making one large flap of the soft palate, and of the soft coverings of the hard. There is one point in M. Roux's proceeding which remains to be noticed: it belongs to the question of accessory incisions. Our readers are probably aware that Dieffenbach practised lateral incisions through the soft palate, parallel to the cleft, with a view to allow its edges to approximate more readily, and that Mr. Fergusson divides the muscles of the palate for a similar reason. Roux, who was acquainted with Sedillot's modification of the latter operation, did not approve of it; he preferred to cut across the attachment of the soft palate to the palatine bones; and we have no doubt, that such an incision, if not carried too far, would facilitate union in those cases in which the operation we have described above for closure of the hard and soft palates simultaneously is not performed. With regard to Mr. Fergusson's, we do not think it necessary in all cases; and if not necessary, then not advisable.

The subject of staphyloraphy has naturally occupied our attention so much, that we can do no more than barely notice the remaining portion of the present volume. The style of the work is such as to render it difficult to formularize either the information or the opinions contained in it. There are, however, a few scattered precepts which are concise as well as valuable. In speaking of autoplastic operations in general, he says:—"Where one flap suffices to fill a vacuum, it is preferable to two; it seems as if nature had less difficulty in effecting the union of one flap than of two; while it is also more readily submitted to the attention and care of the surgeon. . . . The flap should consist of skin alone, or of skin and a fair thickness of muscle; aponeurosis alone forms a bad lining to a flap of skin. . . . In operations, whether by *torsion* or *glissement*, as little displacement as possible should be allowed. . . . If it be necessary to choose between a flap more or less altered by cicatricial tissue, and one of a healthy character, but badly situated, I prefer that which can be placed in situ with the smallest amount of violence." In the letters on hare-lip there is much desultory information of a practical nature: he states his experience of the operation in extreme infancy,—that he has had almost as much failure as success—sometimes no adhesive in-

flammation arose; sometimes ulceration occurred, or violent disruption; frequently fatal fainting, and once fatal hemorrhage; and with regard to the most suitable age, he would prefer, when possible, not to operate before the third or fourth year, especially in double or complicated hare-lip. In double hare-lip, where the central portion is retained, he deems it best to unite the lip independent of it, and by a secondary operation to adapt it to the septum narium; in this way the flatness and deformity of the nose are best remedied.

In concluding our notice of the first volume of forty years of surgical practice, we must say, that to those who choose to give sufficient time and attention to its perusal, it will afford much practical information, conveyed in a manner which, though extremely desultory, is not fatiguing. The addition of a good index or table of contents would render it useful for reference, as the case-book of such a veteran in surgery ought to be. It does not profess to be more, and those who look for anything like systematic surgery in it will be disappointed.

The admirable manner in which the printer and publisher have fulfilled their duties ought not to be passed over without a word of praise.

On Pain after Food: its Causes and Treatment. By EDWARD BALLARD, M. D., &c. London: Walton and Maberly. 1854. 12mo, pp. 136.

IT is the misfortune peculiar to authors on stomach diseases that they can never address themselves to their professional brethren, who alone ought to be the exclusive object of instruction, but they always have an eye turned round on the public: like an Irish barrister, of some celebrity in former days, who, when reprimanded by the judge, who assured him that he could hear him perfectly without his speaking so loud, answered that he cared not whether his lordship heard him or not, but what he wanted was to be heard by those outside the court. The stomach is the organ which, entering into our daily and hourly enjoyment, always interests the non-professional reader; and the author who has seized upon the very symptom which has been felt at some time or other by every human being in our age and country as the title for his book, has done his best to attract a numerous body of readers from among the public at large.

Notwithstanding the unfavourable presentiment suggested by the title, still we proceeded to read the pages of the work

(commendable for its small size), with a hope of finding something to justify its publication, perhaps some new point of diagnosis, or some peculiar treatment hitherto undescribed; but we have met little else than disappointment. We can see nothing worthy of being extracted into our pages. And yet this one symptom, namely, pain after eating, admits of the most decided and practical diagnostic distinction, when viewed with reference to its kind and intensity, its locality, its *adjuvantia* and *lædentia*, the interval of time after eating at which it occurs, and the accompanying symptoms; and the materials for forming such distinctions are not difficult to be found. But Dr. Ballard, without attempting to add anything to the diagnosis, and without taking the slightest notice of many of the most important facts established by others, has rushed before the public, book in hand, as if about to reveal things which, up to the present, had remained hidden in the womb of time, while he has not added a single fact of his own to what was known before. His collection of materials is raked up according as they may be supposed to have a connexion with pain in the stomach; and, like a bad preacher, who is always repeating his text, he has endeavoured to show some coherency between them by means of a digest, to which there are numerical references in the body of the work, in order that we may keep in view the subject on which he has undertaken to write, which otherwise would certainly altogether escape our recollection.

We think that it will be impossible for even the most anxious or atrabilious of his hypochondriacal class of readers to wade through more than a few pages; and as for the members of the profession, we can discover nothing to repay them for the trouble of a perusal.

Pathological and Surgical Observations, including a short Course of Lectures delivered at the Lock Hospital, and an Essay on the Surgical Treatment of Hemorrhoidal Tumours. By Henry Lee, F.R.C.S., Surgeon to the Lock Hospital, &c. London: Churchill. 1854. 8vo, pp. 232.

The work before us contains papers on various points, some of which have already appeared in a different form, and others have not been previously published. The majority are subjects of much interest, and a recapitulation of some of them will show that they are of importance to the practising surgeon. As such, we may select "the causes and consequences of inflammation of the veins;" "the deposit of fibrine on the lining

membrane of veins;" "primary and secondary fibrinous deposits;" "suppuration in bone;" "purulent infiltration of bone;" "fixed and long-continued pain in bone, not dependent upon the presence of confined matter;" "the surgical treatment of hemorrhoids," &c. &c., each forming a separate paper or essay, illustrated by cases, and, in a few instances, accompanied by wood engravings.

The "Lectures" are six in number, the three first devoted to the consideration of "infecting and non-infecting sores;" "local syphilitic disease and inflammatory bubo," and the "means by which the syphilitic poison enters the constitution;" the three last, to syphilization, as applied to animals and man. We shall at present notice only one of the subjects dwelt on by Mr. Lee, and shall select, therefore, as perhaps being not least interesting to our readers, the chapter on "fixed or long-continued pain in bone, not dependent upon the presence of confined matter."

The causes of long-continued pain in bone Mr. Lee enumerates as four, depending upon the different pathological conditions they present, and he classifies them thus:—first, those dependent upon the formation of pus within the bone; secondly, those dependent upon the deposition of more solid material, arising from the poisons of mercury and syphilis; thirdly, those dependent upon a collection of tuberculous matter in bone; and fourthly, those dependent upon the presence of a necrosed portion of cancellous structure."

This list we do not believe is intended to be given as embracing all the causes of pain in bone; for neither neuralgia, nor the polynucleated cell-growth described by Robin as found between the external surface of the marrow and the internal face of the canal, is here noticed; and we know full well the pain, the almost agony, which occasionally accompanies these affections, and which, we conclude, must be equally familiar to our author.

He prefaces his observations with the remark that—

"The pain which attends suppuration in the interior of a bone is not always in proportion to the pressure exercised by the confined fluid; a very small quantity of matter, contained in soft, spongy bone, becoming a source of irritation, and appearing to determine to and fix in the part some pain depending upon constitutional causes, and which, were it not for the disease in the bone, might have fallen upon some other region. These morbid sensations once established will continue, although not originally produced by the local disease, until that is removed; and even after it has been removed, they will, as if from confirmed habit, show a tendency to

return to their accustomed place. After a time, however, if the cause which has determined the pain to a particular spot be removed, and no fresh source of irritation be present, the symptoms will cease."

To these facts we subscribe, but not to the reasoning in the case, adduced by Mr. Lee; for here the morbid local sensations and the morbid general sensations, are both clearly dependent on the same constitutional cause, namely, a system poisoned either by syphilis, or mercury, or both; and a local disease is consequently developed under circumstances (such as a slight blow, for instance) where in a healthy constitution no ill effects would have ensued. It is, therefore, the previous predisposition of the individual that gives rise to the disease of the bone.

The essential characters of the disease under consideration our author holds to be the same, whether the original cause of local irritation arises from confined purulent fluid, or from a deposit of morbid matter in a more solid form. They are enlargement of the bone, sometimes globular, sometimes ovoid; tenderness on pressure over one or more points, with dull, aching uneasiness by day, changed into deep-seated pain by night, of such severity as to preclude all rest till morning. In regard to treatment Mr. Lee observes:—

"When the source of the continued irritation is thus situated within the bone itself, it seems not unreasonable to conclude that a similar plan of treatment would be available, from whatever cause that irritation may arise; and the results hitherto obtained would appear to lead to the inference that relief may be expected from an artificial opening in several classes of cases.

"Whenever there is reason to suspect that pain in a bone is kept up by the presence of some morbid or foreign matter in its interior, or by the pressure produced by a redundancy of bony deposit, it appears evident that the removal of a piece of the shell of the bone is the rational mode of treatment. An opportunity is thereby afforded at once for the escape of any confined matter, and the tension of the parts is relieved: and it appears not improbable, from the favourable effects hitherto obtained from this mode of treatment, that it may hereafter be extended to the relief of many cases of protracted and obscure affection of the osseous system."

Of the correctness of the author's views on this head we have no second opinion. We have seen so great relief follow from it, that we would advise its adoption in all cases of affections of the long bones of the extremities whenever ordinary treatment had failed, and with even a very moderate enlargement of the bone existing, provided the pain is *severe, fixed,*

and *long-continued.* It is the character of the pain that, in a very great measure, should be our guide.

"In determining where the instrument should be first applied, the most projecting point is to be selected as a rule. The next best is to select the most sensitive spot for the operation; but if there be no one spot peculiarly tender, then the temperature of the skin may be consulted; for it will generally be found to be higher over the immediate seat of the disease than elsewhere. Should none of these indications determine the precise spot at which the instrument is to be applied, and should the swelling of the bone extend for some distance, the opening should be made in a dependent situation."

In cases where it is deemed advisable to perforate a bone, Mr. Lee says:—

"I have been led to prefer the use of a trephine, of a very small diameter, in the first instance. It answers the purpose of an exploring trocar in soft parts, and appears to me to possess the following additional advantages:—A portion of bone is more quickly removed with a small than with a larger instrument, and the opening made requires less time to close subsequently. Should the first application of the small trephine not discover the disease, a second or a third opening can be made without inconvenience. When once an opening has been made into the bone, a flexible probe, introduced and turned in different directions, will generally enable the surgeon to determine what further portion of bone, if any, should be removed. Portions of the affected bone are occasionally very hard, and it may happen that a small segment only of a comparatively large trephine may come upon the softened bone (the remainder of the sawing edge being firmly supported by the condensed and solid structure). The operator will then have an intimation, from the yielding of the softened tissue beneath the pressure of the instrument, of being arrived at the disease; and in cases where a very small portion only of the bone is softened, the trephine may be worked deeper than is intended. Such an accident can rarely happen with a small trephine, with which the degrees of resistance of the different parts of the bone are very readily appreciated. It is quite possible, even with a large trephine, to miss an abscess situated in the interior of a bone; and, considering the time that its application requires, and the amount of bone involved, it becomes a subject of serious consideration whether a second or a third portion should be removed"[a].

We fully agree with Mr. Lee in his observations as to the use of small trephines in the first instance, but having by their aid, or that of a gimlet, ascertained the presence of a cavity, we prefer making an opening sufficiently free to give exit to what-

[a] The trephines recommended by Mr. Lee are about three-eighths of an inch in diameter.

ever matters may be contained, be they of a solid or a fluid nature.

Mr. Lee's Essays and his Lectures are clearly and well written; they emanate evidently from a man who has seen that of which he treats, and we can therefore recommend them to the notice of the profession.

Mikroskopische Anatomie, oder Gewebelehre des Menschen, von Dr. A. Kölliker, Professor der Anatomie und Physiologie in Würzburg. Zweiter Band: Specielle Gewebelehre. Zweite Hälfte. 2. Abtheilung (Schluss). *Von den Harn- und Geschlechtsorganen, vom Gefässsystem und den höhern Sinnesorganen.* Mit 140 Holzschnitten, ausgeführt von J. G. Flegel. Leipzig: Verlag von Wilhelm Engelmann. 1854.

Microscopic Anatomy; or Histology of Man. By Dr. A. Kölliker, Professor of Anatomy and Physiology in Wurzburg. Second volume: Special Histology. Second half: Second Division (conclusion). *Of the Urinary and Sexual Organs, the Vascular System, and the higher Organs of Sense.* With 140 woodcuts, executed by J. G. Flegel. Leipsic: William Engelmann. 1854. 8vo, pp. 438.

In a former number of our Journal[a] we had occasion to notice the first half of the second volume of the above important work, treating of the skin, muscles, bones, and nerves, of the human body; as well as the first division of the second half, embracing the histology of the digestive and respiratory organs. The portion now before us is, in the ability with which the several subjects are treated of; in the acquaintance with the labours of others displayed in it; and in the amount and value of the contributions of the author himself to histological science contained in its pages, well calculated to maintain undiminished the justly merited and world-wide reputation of the writer: while in the beauty of its typography, and the excellence of the artistic illustrations, it is fully equal to the Parts which have preceded. The title, as we have quoted it, sufficiently explains the contents and arrangement of the present section of the work: we shall, therefore, proceed, without further preface, very briefly to allude to some of the points which may strike us, in passing through the volume, as being, from their practical impor-

[a] Vol. xiii. of our present Series, p. 432.

tance, novelty, or interesting nature, worthy of more especial attention.

We shall first bring before the reader the author's description of some of the pathological changes to which the tubuli uriniferi are liable. These little vessels consist, throughout, essentially of the same elements, namely, a proper membrane (*membrana propria*), and a pavement epithelium. The former is a completely structureless, transparent, thin, but comparatively firm and elastic envelope, the normal thickness of which does not amount to more than from 0·0004 to 0·0008, but is often, under certain morbid conditions, increased to 0·001, and even 0·002 of a line, when it frequently exhibits on its inner surface very beautiful, closely-placed, delicate transverse striæ. The epithelial cells, especially those of the cortical substance, often contain fat globules in considerable quantities, so that they might be mistaken for cells taken from a fatty liver, particularly as they are generally at the same time enlarged to 0·02 of a line. The normal measurements of the epithelial cells are stated by the author to be, in the convoluted tubuli, from 0·008 to 0·012 of a line in breadth, and from 0·004 to 0·005 in thickness, while in the straight tubuli they are only from 0·004 to 0·006 broad, by 0·004 in thickness. With the fat we also observe pigment granules (of the colouring matter of the urine?) in the convoluted and also in the straight tubuli; while, on the contrary, the concretions of urates and calcareous salts, which so often occur in the cavity of the tubes in vertebrate animals, have not as yet been with certainty demonstrated in the cells (in fishes Simon often found crystals in the renal cells). Bright yellow colloid-like masses are frequently discovered in the epithelial cells, which are then, for the most part, enlarged to form narrow cysts, from 0·05 to 0·072 of a line in length; and, finally bursting, discharge their colloid masses, which are likewise enlarged, causing the latter to be found free in the tubuli uriniferi, and also in the urine. A development of the epithelial cells to form other cysts, as assumed by J. Simon and Gildemeester[a], has not, the author states, as yet occurred to him; on the other hand, he observed, as Johnson has done, an evident degeneration of the convoluted tubuli in an atrophied kidney into closed cysts, taking place, to all appearance, by means of an areolar tissue, developed between the tubuli and strangulating them, which, while they retained the same structure as the tubuli, were in some parts of equal width, but in others were dilated to form vesicles of 0·1 of a

[a] Tijdschr. d. Nederl. Maatsch., 1850.

line in magnitude. The Malpighian corpuscles may also be dilated to form cysts, in which, with a clear fluid, the atrophied corpuscle (glomerulus) is often to be seen attached to the wall of the cyst.

As abnormal contents of the tubuli uriniferi we observe:— 1. Blood, most frequently in the commencements of the convoluted tubes, particularly those of the surface, and often in such quantity that particles of blood, as large as the head of a pin, occur visible to the naked eye: these were formerly incorrectly looked upon as dilated Malpighian bodies. 2. Fibrine, in cylindrical masses, corresponding to the canal of the tubuli. 3. The above-mentioned colloid-like substance. 4. Concretions in the tubes of Bellini, consisting in the adult principally of carbonate and phosphate of lime (calcareous infarction), and in infants, of urates (uric acid infarction of Virchow), which latter give the pyramids a beautiful golden yellow colour, and occur, if not exclusively, at least generally, in children who have respired (between the third and twentieth day). In Bright's disease, many of the tubuli, which, through the exudations that have taken place in them, have lost their epithelium, become in the latter stages of the affection atrophied, and finally wholly disappear; while groups of others, enlarged and filled with fatty exudations, appear in the form of little nodosities (the granulations of Christison).

In the section on urinary deposits we have the author's view of the nature of the pellicle, which has received the name of Kiestein. Professor Kölliker shows that, with the exception of occasional fat globules, the occurrence of which Lang has fully proved, normally excreted urine contains no morphological element. It is only casually that we find in it epithelial cells from the urinary passages, particularly the bladder and urethra; almost always we have mucus proceeding from the same parts, and forming a cloud or light sediment, with occasional mucus-corpuscles; finally, after seminal emission, we observe spermatozoa. In inflammations, hemorrhages, exudations, and the formation of fat in the kidneys, we discover pus, fat, and blood corpuscles, coagula of blood or fibrine, in the form of cylindrical plugs, casts, in fact, of the tubuli uriniferi, and epithelium, from these tubuli, either isolated or forming coherent strings or shields. All normal urine, not depositing a sediment, passes, if kept for some time at a medium temperature, under the influence of the mucus contained in it, into a state of acid fermentation, during which filamentary and fermentation fungi appear, and lactic or acetic acid is formed by the decomposition of the colouring matter of the urine, uric acid

being consequently set free. Sooner or later the acid disappears; the urine becomes ammoniacal and alkaline from decomposition of the urea, and perhaps, also, of the colouring matter, and large, colourless pyramidal prisms, or stellated needles of triple phosphate, soluble in acetic acid, make their appearance, which, mixed with numerous infusoria (vibriones and monads) give rise to a superficial pellicle, and, with granules of urate of ammonia and carbonate of lime, constitute a white sediment. In affections of the bladder the urine often passes immediately into alkalescence, with the formation of the crystals of triple phosphate, which latter also very frequently occur in the urine of pregnant women, and at first in the form of the pellicle, above described, were considered to be a peculiar substance (Kiesthëin).

The importance of these observations of Professor Kölliker was impressed upon us a few days ago by there being submitted to us the urine of an unmarried female, whose abdomen was enlarged, and in whom pregnancy was suspected, because a greasy-looking pellicle, supposed to be kiestein, was observed to form on the surface of the urine after a few days' repose. On closer inspection, sparkling crystals of triple phosphate were plainly visible to the naked eye; and on examination with the microscope, the pellicle was seen to consist of prisms of ammoniaco-magnesian phosphate, surrounded with an enormous number of vibriones, among which numerous monads were moving about with much activity. It is needless to add, that there is no necessary connexion between such a condition of the urine and pregnancy, nor did the latter state in this particular instance exist, the enlargement of the abdomen having been found to depend upon ovarian disease.

The author makes some remarks in reference to the occasional occurrence in the breasts of infants of a small quantity of a fluid resembling milk both in its external and microscopic character, to which we may briefly allude. The origin of this fluid is probably connected, he thinks, with the formation of the glandular ducts, just as he supposes that of the colostrum to be connected with the maturity and final formation of cavities (follicles and ducts) in the more central portions of the gland. The morphological elements of the latter fluid he supposes to be nothing but the inner cells of the last milk follicles and lactiferous tubes developed during pregnancy, filled with fat; and he remarks that the process which we find to take place at the period of the first lactation is not confined to that time, but may be observed during the entire development of the gland, appearing energetically exactly when this organ

makes greater degrees of progress. Such a period, he continues, is that immediately after birth, and in the first year of life. It has, he adds, long been known that the mammary glands of children, of both sexes, often become remarkably swollen, and on pressure discharge a milky fluid, which is also frequently to be found, in tolerably large quantities, on making a section of the gland in the dead body.

The microscopic investigations of several observers show that this infant milk has sometimes the properties of true milk, at others, those of colostrum; and it might hence be inferred that in this instance a real secretion, whether normal or pathological, takes place. "But, in my opinion," continues Professor Kölliker, "the entire phenomena may be more simply referred to the more rapid progress of the development of the gland after birth; and I think we may venture to assume, that the appearance of milk in this case is connected with a formation of fat in the central cells of the still solid parts of the gland, by which these latter first acquire their cavities. Consequently, we should here have to do with a physiological process, which, on the one hand, frequently appears with but slight intensity, and then passes over unobserved, while, on the other, it may lead to congestions and inflammations of the gland, and then seems to be a purely pathological occurrence, like the formation of milk in various diseases of the breast and in uterine affections.

"As in infants and children, so also at the period of puberty, when the gland likewise makes considerable progress in development, a milky fluid, with colostric bodies, may be found in the lactiferous ducts, and I therefore think that my view of its origin is tolerably well borne out. I do not, however, mean to say that an actual secretion does not coincide with the development of the glandular ducts; I am of opinion that this in fact occurs in children, and in women pregnant for the first time. This secretion would then, in a woman pregnant for the second time, by itself form the colostrum, as it is scarcely to be supposed that new lactiferous ducts are formed in every pregnancy."

The writer of this notice had some years ago an opportunity of examining about half a fluid drachm of a milky fluid taken from the breast of a male child, three weeks old, and furnished to him by Dr. Battersby; it was alkaline, threw up a cream, presented under the microscope normal milk globules, of various sizes, on the whole smaller than those of the adult female; a very few colostric bodies, with some epithelium, and some masses of dark colouring matter, were also observed. Water

of caustic potash did not render the fluid viscid, nor did the milk coagulate when boiled. Dr. Battersby mentioned this case at a meeting of the Surgical Society of Ireland, on which occasion he expressed his regret that, from the smallness of the quantity supplied, and the fact of the experimenter's attention having been chiefly directed towards ascertaining whether the colostric bodies, seen under the microscope, were sufficient in amount to give the specimen the chemical properties of colostrum or not, the presence of casein, which would have fully established the identity of the fluid with woman's milk, had not been determined. This deficiency has since been amply supplied. M. Natalis Guillot observed milk taken from the breasts of thirty-nine male and thirty-four female infants; he considers its secretion to be a normal function, as it only occurs in healthy children. The fluid was white, neutral or alkaline, and became acid on exposure to the air; it separated into a serous and a creamy part, and possessed the same composition as the milk of the adult female, casein, sugar, and fat, having been found in it. The milk, he adds, is a perfect one[a].

Dr. Schlossberger examined "a specimen of the white milky fluid, often secreted by the mammary glands of new-born infants, of both sexes, obtained from a boy, who yielded about a drachm during the course of a few days. The fluid had the appearance of milk which had been watered; it reacted alkaline, and exhibited under the microscope the normal milk corpuscles. It reacted strongly upon sugar, but did not coagulate by heat; it did so, however, upon the addition of acids and rennet. It consisted, in 100 parts, of water, 96·75; fat, 0·82; ashes, 0·05; casein, sugar, and extractive matter, 2·38. It was, in fact, a very thin, but truly chemically constituted milk"[b].

From the results of the foregoing examination there can, we think, be no doubt that the fluid in question is, in great part at least, the product of an actual secretion by the mammary gland of the infant.

We should gladly draw more largely from the pages of Professor Kölliker's valuable book; but to do so fully to our satisfaction would require a complete analysis of the volume, an impossibility in our pages. We may again return to it, and in

[a] Gazette Médicale de Paris, 29th October, 1853, p. 686; also Archives Générales de Médecine, November, 1853, p. 513.

[b] Würtemb. Corr. Bl., 1853; and Medical Times and Gazette, November 19, 1853, p. 537.

the meantime most strongly recommend all of our readers engaged in microscopical investigations to refer to the original, which will afford them a rich mine of information in their pursuits.

Practical Observations on Mental and Nervous Disorders. By Alfred Beaumont Maddock, M. D. &c. London: Simpkin, Marshall, & Co. 1854. 8vo, pp. 232.

There are books we read for profit, books we read for pleasure, and books from the perusal of which, while deriving the one, we also experience the other. A book, to be of value as a scientific work, to occupy an honourable and lasting position in the archives of that temple wherein are registered the votive offerings of truth-loving men, requires to be based on a surer foundation than that of mere rhetorical display, which, however indicative of a generous imagination, cannot be received as evidence of mental vigour. Far be it from us to insinuate that "refined and profound thought" may not, as Cicero has observed, be expressed in "soft transparent diction." We rejoice to say that many of our most deservedly esteemed scientific works are as equally distinguished for purity of style and harmony of expression, as for correctness of description and justness of inference. It is fitting that it should be so, when Philosophy and Medicine, joining hand with their fairer sisters, Eloquence and Learning, visit their native mount together, to offer tribute at their patron shrine. Great thoughts have their natural garments distinguished rather for their quality of texture than richness of colour,—their aptitude of design than elaboracy of construction. Philosophers should ever eschew foppery, as seldom failing to detract from their natural dignity. We wish to impress this fact, as we are each day convinced that sentimentalism is not confined alone to romantic girls or aspiring youths. Erudite physicians join in their childish games, and send forth Medicine with her sombre form disguised in all the flimsy whimsicalities of modern Bloomerism.

We know of no reason why physicians should not cultivate their tastes for polite literature, and aspire to the highest excellence in all those refinements of intellectual life, for which the peculiar nature of their studies and daily exercises so pre-eminently qualify them. The great scientific and literary institutions of our country are proud to number amongst their most distinguished and useful members, individuals who, from

the anxieties and responsibilities of practice, snatch a few moments for such relaxation as finely-wrought minds alone can indulge in. There is a suitable time and occasion for all things! What would be thought if some one physician, more aspiring than his fellows, published his clinical lectures in rhyme? We leave the question to the consideration of our readers. Stranger matters have occurred ere now. Despite the warnings of Coleman and his detail of the sad consequences following the directions,—

> "When taken
> To be well shaken,"

we do not altogether despair of such a treat, for, judging from the introductory chapters of the work before us, and contrasting them with others we have recently been favoured with, it appears that an extensive acquaintance with the poets of the day forms no inconsiderable item in the requisite qualifications for modern medical authorship. A question naturally arises,—with what objects are professional works written? Is it to afford to those engaged in similar exercises the well-considered results of careful observation, which may in the hour of danger guide the doubting, and impart additional confidence to the anxious labourer in the cause of suffering man? If so, away with those embellishments which mock the seriousness of such deliberations. Does an author desire his work should outlive his memory, and entitle him to be marked amongst those who deserve well of his fellows? Let its eloquence rest in the force of its reasoning, its romance consist in the identity of its descriptions with the mysterious operations of life. Be this as it may, the fountain of medical science must be preserved both pure and free. Its waters are life-giving streams, be ours the care they flow undisturbed, that truth be not disguised in its progress, or disfigured in its course.

We do not wish to speak disparagingly of the volume before us because its author has therein dealt forth poetic quotations with a right liberal hand; though, if our memory serves us right, the Venusian bard long since wrote,—

> "Quod medicorum est
> Promittunt medici."

Had he lived in the present day, to define the foregoing might prove a matter of no small difficulty. We confess ourselves as being but indifferently calculated to estimate the scientific application of those "elegant extracts," nor can we, at the risk of being considered a copyist of Dr. Maddock, avoid, as

we peruse them, recalling the personal experience of Tom Moore respecting their source and utility:—

> "Read at a stall, for oft one pops
> On something at those stalls and shops,
> Which does to quote, and gives one's book
> A classical and knowing look."

Dr. Maddock informs us in his preface that, in addition to the ample opportunities which public appointments have afforded him, much experience among private patients has also furnished materials for the composition of his present work; notwithstanding which he writes:—

"To either novelty or depth of research, the author, in the composition of these pages, makes no pretence; for he is thoroughly aware that many of the deductions and inferences herein contained were well known to the ancient writers, though of late too much disregarded; but he may affirm that they are the result of reflection and considerable experience. The work is submitted rather as 'materials for thinking,' and as forming a *point d'appui*, as it were, for further extension by abler or more leisurely hands, than offered as a finished production."

Certainly the latter part of this admission sounds strangely to our ears, more particularly as we have so recently noticed in our pages many "finished" volumes from the pens of such men as Stokes and Todd, who, amid the toils and anxieties of the largest practice, still found time to perfect their labours in the cause of science. We cannot regard this statement otherwise than one savouring strongly of the "pride which apes humility," inasmuch as the term "more leisurely" delicately removes any difficulty which the non-discovery of "abler hands" might originate. We have no wish, in the commencement of our review, to question the author's justness of opinion, or discriminating appreciation of his own work, respecting which he is so pre-eminently qualified to judge; especially as its careful perusal has fully satisfied us of both its candour and truth. Such being conceded, it may not be unreasonable to ask, with what object has another been added to the list of *ordinary* works on a subject to which so much close study and *extraordinary* attention has been, and now is, being bestowed? By what standard are we to estimate the merits or value of a new work? whether by the relation of its contents to our admitted knowledge on those subjects of which it professes to treat; or according to the value we attach to the opinions of its author, from our acquaintance with the opportunities he has enjoyed for perfecting his judgment? By neither of these

criteria can we for the future be guided, since it matters not what their opportunities be; though feeble men could not write great books, able men may write unfinished ones, so that the want of ability on the part of one, and leisure on the part of another, so equalizes their labours, that their works are thereby closely assimilated to the same standard.

The varied observations contained in the first three chapters of this "new work on affections of the nervous system" are certainly more distinguished for their florid style than original application; for their musical sound rather than their scientific matter. Old truths are re-stated; old authors again quoted. The apposite remarks of "that *elegant* Roman poet, Horace," are not forgotten, who, could he be conscious of the estimation thereby expressed, we doubt not, would feel both flattered and proud, notwithstanding the extraordinary liberty subsequently taken in applying his eulogy of Pyrrha, as expressed in the phrase, *simplex munditiis*, to a patient labouring under a disease of the skin, and translating it,—"elegant by cleanliness,"—at which wide range we have been not a little amused. The first three chapters are, however, but the prelude to the more important matter, inasmuch as they conclude with a classification of the various nervous and sympathetic affections most frequently met with in practice. This classification is as follows:—

1. Affections of the brain and nervous system, associated with morbid states of digestion and assimilation.
2. Affections of the brain and nervous system, connected with derangement of the circulating and respiratory organs.
3. Sympathies of the brain with the function of locomotion.
4. Sympathies of the brain with the reproductive and urinary organs, and the functions of sense and sensibility.

Dr. Maddock purposes to consider each of those classes in more particular detail, and we accordingly accompany him in his progress.

The fourth chapter of this work, constituting the most extensive as well as most important portion of the whole, is occupied by the consideration of the first of these divisions.

We pass Dr. Maddock's very meagre observations respecting the sympathetic system, in which the well-known capability of visceral irritation, through reflex nervous action, to induce morbid and unnatural cerebral excitement, is set forth as the rational foundation on which the treatment of such excitement, to be successful, should be based. This we conceive to be a matter of such ordinary experience, that any further

remarks in reference to it would be superfluous. Our author, however, thinks otherwise, for he thus writes:—

"As the medical officer, for some years, of an extensive lunatic establishment, I have necessarily enjoyed great opportunities in the observation and treatment of diseases of the brain, in all their numerous and protean forms; and it is one of the chief objects of this work to show, that they have not that indelible organic character which has generally been assigned to them, but that they are chiefly dependent upon a deficient nervous energy, and more especially connected with the digestive, assimilating, and excretive organs."

We would ask Dr. Maddock for what class of readers is this work designed. If for the public, we protest against his propagating so unfounded an assertion, as may be inferred from his declaration that indelible organic characteristics have been assigned to mental diseases by those whose opinions are worth canvassing. If it be intended for the profession, we cannot but admire the coolness and self-possession evinced by its author in undertaking to prove that, of which we are satisfied the veriest tyro in psychological investigations does not entertain the vestige of a doubt.

We have carefully perused this chapter, abounding with quotations from "illustrious" and "classic" "authors," "noble poets," "quaint old writers," "great moral philosophers," "great lexicographers," "acute," "sagacious," "intelligent" physicians, and "accurate anatomists." We have been impressed with Dr. Maddock's intimacy with "Lord Bacon, who was a physician as well as a philosopher, in times of old;" with "the witty Lord Chesterfield," and with "the words of immortal bards." We have had our recollections of the poets, ancient and modern, amended and improved, but we have *not* had one iota of useful knowledge added to that which other observers had, with honest seriousness and truthful earnestness, previously, fully and fairly set forward. We do not by this mean to convey, that those many desperate cases which recovered with railway speed were not treated well and judiciously. In stating thus much, and allowing that average ability was manifested in their management, we concede all that critical justice will permit; for, while seeking in vain amongst them for the promised *point d'appui*, we have felt humiliated that their details were not more carefully revised previous to publication. We extract the following. Referring to a gentleman's removal to the author's asylum, we read:—

"During the journey he was extremely dejected, continually

exhibiting fears that I was 'taking him to a gaol,' and that he had 'lost his commission,' &c. Finding upon his arrival at my establishment that his surmises about a 'gaol' were unfounded," &c.

Again, in another example:—

"This case (which is well known to Dr. Hayman, of West Malling, and to Mr. Saunders, surgeon, of Tenterden) had been rejected at Bethlem Hospital as being incurable."

Again:—

"Having a large family, and a sick child, it would be very inconvenient for me to leave home, and therefore hope that you will be enabled to satisfactorily treat my case by letter. I am encouraged in this hope by finding that it was by epistolary correspondence that you restored Mrs. C—— from a state of miserable mental despondency, and bodily suffering, to perfect health. . . . . I say *ameliorated*, for I despair of being cured, having been so long under treatment to no purpose."

This patient is reported to write thus at the end of seven weeks:—

"I have continued to improve daily since I last wrote, and am in excellent spirits. . . . . When I bring back to my mind the horrors I used to endure, I cannot sufficiently thank you for your kind and successful efforts to restore me; and I also feel grateful to my dear friend in having so fortunately recommended me to seek your aid."

Dr. Maddock, reporting a case of hypochondriasis, and detailing the mental and bodily suffering of the patient, writes:—

"He further added, that the former had been maintained and aggravated by the unfeeling manner in which his family, his friends, and even his medical attendant, had derided and ridiculed his complaints."

Another patient, having detailed his extreme case, adds:—

"It appears to me that I cannot do better than place myself under your treatment, seeing how successful you were in the case of my dear niece; we all feel that we can never be sufficiently grateful to you for her recovery."

"An instance of this kind occurred to me about fourteen years ago, in a person (known to Mr. Karstadt, of the General Post Office)."

We might extend this list of quotations by many others of a similar class. Our readers are, we doubt not, as weary of them as ourselves. Their parallel can only be found in those fulsome and contemptible puffs which are set forth by many,

and deservedly regarded as being equally derogatory to science as they are injurious to that high character of the medical profession we are each called on to sustain. We regret that any work emanating from a gentleman of Dr. Maddock's apparent position should abound with so many just grounds for censure. Writings of this nature we shall always consider it a duty to publicly censure, and energetically condemn, even though flashes of genius illume their pages: much more when commonplace truths are thus submitted to the profession and the public, their declared philanthropic origin shall in no wise lessen the offence.

A Disquisition on Certain Parts and Properties of the Blood. By David Tod, M. R. C. S. London: Churchill. 1854. 8vo, pp. 263, with illustrative woodcuts.

We have a tendency, it may be an idiosyncracy, to judge of the general style of a book by the character of the preface. This estimate is not influenced by the length or brevity of the introduction, but rather by the presence or absence of those ideas which should constitute the prelude to the enunciations which are to follow. It may possess some features of novelty to those uninitiated in the mysterious science of literary anatomy to learn the usual sequence adopted by authors in the construction of preface and volume. The latter is invariably completed before the former is commenced,—the prefatory introduction being composed as a text to the volume, or a didactic expression of its contents, written while the author's mind is tinged with those ideas which he has portrayed at large in his work. There is one character, however, which we esteem to be essential to its utility, namely, that it should be clear and intelligible to all possessing ordinary understanding, for such we assume to be the standard of psychical endowment that nature has allotted to the majority of our species. There are certainly a few exceptional cases occurring in every age, where nature has with an unmeasured prodigality invested men with the most exalted development of intellectual attributes, evidenced by peculiarities so striking, but at the same time so dark and obscure in their sources, that to avoid a difficulty in the application of a suitable expression, we select one which cannot be defined, naming the gifted individual *a genius*. But notwithstanding the inspiration of this peculiar gift, it frequently occurs that those who possess it in reality and in fact, as well as those conceiving that they are thus favoured, are so

obscure, not only in their writings, but even in their conversation, as to be perfectly unintelligible to ordinary mortals. It is for this reason that it were better for such men to indulge in their visionary speculations, and exercise their peculiar diction in the recesses of solitude, than seek to obtrude their *ideals* on a practical and utilitarian world, with objects above the influence and beyond the power of a dreaming system of philosophy, created by a weak indulgence in those uncontrolled flights of fanciful imagination, which terminate eventually in the perversion of all rational and useful ideas. But if it is one of the necessities attached to our social condition, that these literary manifestations of genius must occasionally appear to strike with wonder, surprise, and awe, those inferior mortals who are doomed only to the possession of ordinary capabilities, it is well that the cold, stern, and unpoetic mind of the critic guides the hand, which, wielding its disenchanting wand, shivers the gilded palaces and temples that the visionary has created, and strikes terror and devastation throughout the regions of his pictured dreams.

We regret that so unpleasing a task has devolved on us in the present instance; but whilst expressing an indisposition to condemn the labours of others, we will not express a single opinion without a just cause being assigned, and a valid reason given for our condemnation. Mr. Tod states in his preface, that he began his inquiries—

"By investigating the process of germination in the various kinds of vegetable seeds, and of the hatching of the egg of the common fowl, and by ascending to the changes which take place in the various periods of respiratory life, to endeavour to discover, if possible, the laws which govern the economy of man. Before proceeding far with these inquiries, the subject became deeply interesting; for the further the economy of nature was examined, the more imperfectly her laws seemed to be understood in very many of her daily operations. *The most opposite phenomena were unfolded in the most perfect manner by apparently the same means, and the development of every vital entity became associated with its most remote periods, and had a direct relation to general laws.*"

We are utterly at a loss to understand what Mr. Tod can mean by these expressions, and although he may have fully understood what he wished to convey, yet to us they seem merely a number of words void of a single intelligible idea.

The volume consists of six chapters, but we will not subject our readers to the infliction of a critical review of each, preferring rather to select a few striking passages from the work sufficient to determine its value; whilst we add, for the sake of

explanation, a brief, but pointed commentary, to prove the justice of our opinions and eventual conclusions with respect to the position which this literary effort should occupy in the physiological annals of the nineteenth century.

The first chapter treats of the blood discs, including their physical and vital properties. The author starting with the assumption of a new designation—Hæmatozoa,—conceives them to be animalcules, possessing inherent motile power and evident electrical attributes; but unfortunately the proofs on which these assertions are based seem to have lapsed from the author's memory, for on no other grounds can we account for their omission,—the only attempt to support an opinion so novel in its nature, and startling in its assumed results, being the observations of Mr. Tod with the microscope. He states—

"That in observing the circulation in the capillaries, he has seen the blood discs (hæmatozoa) assuming a spindle shape;" and again:—"Twice I have seen the smallest extremity of one of these erratic bodies move like the tail of a fish swimming; and once I saw, when one of these bodies was turning over slowly, and apparently changing its form, in the colourless liquor sanguinis, *shades of convoluted* structures, like intestines, where nothing but a limpid fluid was visible, whilst the body retained the discoid form."

Who that has used a microscope, even occasionally, is not aware of the change occurring in the shape of the corpuscles? Not one, we firmly believe; although they may be unconscious that Mr. Gulliver has specially drawn attention to the fact. That it is the result of a physical cause, and in nowise an evidence of a vital characteristic, is rendered fully apparent by continuing the observation on the corpuscles which have suffered change. They will be seen in every instance to return to their former circular figure when the compressing agency is removed, and this very resiliency is taken as a proof of the elastic nature of their globuline element. If Mr. Tod will subject the web of a frog's foot to the stimulating influence of a strong solution of chloride of sodium, and then steadily watch the discs becoming aggregated, crushed, and crowded, we promise that he will see far greater changes than those which he records, and almost predict that a supplement to the present work will eventuate from this simple experiment.

In pages 26, 27, 28, the author enunciates one of the most startling opinions that the most vivid imagination could have conceived. The celebrated Ruysch, who achieved so much by his successful injection of the structures of the human body, believed all glands to consist of vessels, variously modified, or,

in other words, that the secerning capillaries and efferent ducts were continuous. Now the seminiferous tubules are about four times greater in diameter than the capillaries, which would seem to preclude the possibility of an anastomotic union, and observation has proved that both the vessels and ducts are perfectly closed; yet, notwithstanding these obvious circumstances, Mr. Tod states seriously, that the blood discs are changed into spermatozoa. He says:—

"These observations lead to the conclusion, that the hæmatozoa and spermatozoa are merely the primary and secondary stages of the same being;" and again: "That in this condition, hæmatozoa are attracted by and conveyed to the seminal vesicles from the spermatic arteries, and in their passage changed into spermatozoa."

At one fell swoop the whole theory of cellular secretion is destroyed, and the labours of Schwann, Schleiden, Kölliker, and Goodsir, rendered nugatory; Wagner, Müller, and Henle, in describing the absolute formation of spermatophori with their included spermatozoa, must indeed have indulged their imaginations more than exercised their visual organs if the metamorphosis of the author is admitted to be correct. Now what are his proofs to induce physiologists to recognise his views? These we propose briefly to examine: first as to the blood discs escaping into the seminal vesicles, and in their passage being converted into spermatozoa. If Mr. Tod will examine the straw-coloured fluid of the vesiculæ with a good microscope, it will perhaps astonish him to discover that the presence of spermatozoa constitutes the exception, and not the rule. We have never seen a single trace of them in numerous examinations of the fluid; this convoluted tube representing a vesicle is not a receptacle for semen, as Hunter long since explained, and the anatomy of the generative apparatus in the elephant fully proves. It is simply a separate secreting surface, ancillary but not essential to the elimination of the fluid elements of the semen.

The second fact adduced to justify the supposition of metamorphosis is, that the discs and spermatozoa are of the same figure in the human subject, both oval or elliptical. We were not aware that the blood discs were oval in man, having always seen them when freely passing through their natural channels of a distinctly circular figure, an opinion confirmatory of that of Gulliver, Müller, Hewson, Jones, and others. How Mr. Tod could have made this unfortunate lapse seems almost unaccountable, unless that his observations were made on the blood of the members of the class Camelidæ, which are the

only mammals possessing oval discs, and in this respect resembling reptiles, birds, and fishes.

The third proof amounts to this, and no more: that because physiologists have not seen a blood disc collect all its energies for a vigorous effort, and rush incontinently against the vascular walls which imprison its unwilling spirit, and, failing in the attempt, retire defeated and discomfited, it is just, in the absence of the negative, to assume the affirmative proposition. Here is the extract, in order that we may avoid the charge of misrepresentation:—

"To these objections I may reply, that although no communication has been or can be traced between the spermatic arteries and seminal tubes, that circumstance does not prove its non-existence."

The second section treats of the blood as a *conditional compound*, implying that certain relative and uniform connexions subsist between its different elements constituting a state of integrity of the mass. We are tempted to indulge in an extract from the commencement of this section, which will indicate the nature of that which follows in the subsequent pages. He says:—

"It is necessary to notice the different constituents of the blood, as this has been done by others; now it is requisite to compare the different constituents of blood and chyle with one another, for whatever exists in blood, and not in chyle, must be due to the functions of the hæmatozoa, and to its electrolysis in the lungs and peripheral vessels, as will be shown hereafter. It is not necessary here to investigate the electrical condition of the blood constituents either in their normal or abnormal state; for every constituent of the blood is a compound, of which the elements are arranged and held together in definite proportions by a certain amount of cohesive force, and all are bound up with and accompanied by definite antagonistic electrical conditions. It may, however, be observed that every constituent of the blood must be changed and prepared for its ultimate uses before any portion of it passes from the main current into any exhaling and absorbing vessels; and every one of these vessels must attract and remove matter in accordance with its special function. Therefore, in every abstraction from the blood, the abstracted matter must be *anion*, and the blood *cation*. This seems to be the case with the blood and its vessels in all parts of the body. At the electrodes of the vascular circuit, where the blood undergoes general changes, it becomes *cation* the moment it commences flowing through the capillary extremities of the arteries, and *anion* the instant it enters the anastomosing capillaries of the veins, and in this condition it flows as a whole to the heart, where the same phenomena recur. In every diastole and systole of the auricles and ventricles, each cavity

is alternately *anelectrode* and *cathelectrode*, and the blood alternately *anion* and *cation*. In both arteries and veins, the blood, notwithstanding the difference in its colour, capacity for caloric, temperature, and properties, is therefore *anion* in its vessels, until it reaches their respective termini or the mouth of an absorbing vessel, where it becomes *cation* for the support of the vital functions."

We confess ourselves utterly unable to give any adequate explanation of a theory so ingenious and novel, and, therefore, prefer presenting this extract to our readers, devoid of any commentary, which must necessarily fall far short of the merits implied by so beautiful an exposition of the electrical attributes of the blood in relation to the vital functions.

The second Chapter treats of the colour of the blood, the author detailing the composition of light and colours, with some remarks as to the psychical appreciation of their manifestations; and in Chapter III. the actions of the blood are discussed at large. These questions, together with the succeeding chapters, we must leave untouched, and pass rapidly to Mr. Tod's views of the "etiology and pathology of the blood." Taking into consideration the labours of Andral and Gavarret, Rees, and others, on the subject of hematopathology, we were prepared to anticipate at least a good digest of the opinions of previous writers, with the author's commentaries, and, perhaps, additions to our previous knowledge on the subject; but even here we were doomed to suffer a severe disappointment, and compelled to peruse a collection of irrational and delusive hypotheses totally foreign to the subject, and only calculated to lead to the most erroneous conclusions.

Having exhausted his favourite theme of the influence of heat in modifying the blood and inducing disease, he next indulges in some startling statements in reference to therapeutics; ridicules the influence of the secale cornutum over the gravid uterus; denies the utility of cod-liver oil, and recommends, as a substitute, fat beef and mutton, grounding his objection to the latter medicine on the fact that *cases of phthisis did absolutely prove fatal*, notwithstanding the exhibition of the oil even in large quantities.

Before we conclude our notice of this work we are anxious to select a case or two, in order that our readers may form an idea of the author's mode of making a record public. At page 261 we find the following:—

"Charlotte Hassell, aged fourteen years, residing at 5, Pitt-street, Charlotte-street, Fitzroy-square, was placed under my care on the 25th of May, 1852. About six years before this period she was observed to start and jump whenever she went to bed, and continued

to do so occasionally until she fell asleep. Continued in this state about a month, and then the disease began to appear frequently during the day. About five months after the first appearance of the malady she became a patient to the Dispensary in King-street, Golden-square, under Dr. B.; continued under his treatment about eight months, but obtained no relief. A lapse of twelve months then occurred without applying for further advice; but becoming a great deal worse, she was made an outdoor patient to University College Hospital, under Mr. Q. Continued attending the hospital nearly one year, and regularly took one kind of medicine, which consisted of a red powder, but could not tell its name. It made her motions very black, but gave her no relief. She then ceased attending the hospital, and applied for no further advice for two years, as her father considered the disease to be incurable. At the expiration of that time she became very ill, and was made a patient to the North London Dispensary, New-road, St. Pancras, under Dr. A. Continued attending the physician for three months; but getting worse, and rapidly losing flesh, she ceased attending any longer, and remained at home, without having further advice, for several months. On becoming alarmingly ill, she was placed under the care of Dr. T., of 51, Charlotte-street, Fitzroy-square: obtained no relief, and discontinued taking any medicine for some time. At last her head became covered with sores, and a mass of animated nature. An order was procured to get her into the workhouse, there to end her days. This brought her under my notice. She was then in a deplorable condition, scarcely able to stand without support; could not prevent her head from being drawn backwards every two or three seconds, nor her body from being moved in various positions; and in attempting to walk, her legs were forcibly drawn up every two or three seconds, as if to kick something behind her. As the disease was evidently chorea Sancti Viti, and feeling an interest in the case, I declined sending her to the workhouse infirmary, and placed her under treatment. She was put at once under a course of emetics. Three days afterwards she was able to walk to my house without assistance; all the vermin in her head had disappeared under the application of the usual remedy. She regularly visited me once a week until July 14th, when her general health was so much improved that her father considered her cured. On January 15th, 1853, I visited her, and perceived that the disease was not eradicated; but as it did not prevent her from attending to work, nor give her any annoyance, and as her general health was good, the father did not deem it necessary to apply to me again."

We wish to draw especial attention to this fact, that Dr. B. failed; Mr. Q. was unsuccessful; Dr. A. unfortunate; and Dr. T. useless: yet the moment our accomplished author took up the case, after this successive series of failures, the girl at once improved, and in *three days* was able to walk without assistance. What a pity that she had not the good fortune to have applied

to Dr. Tod at once, and then she would have escaped so much suffering, and her four charitable doctors Mr. Tod's censure, which is implied, if not absolutely stated.

From the general tenor of our review it may be inferred that we entertain an unfavourable opinion of the author's capacity, but such is far from being the case. There are few pages of the book which do not evidence the close application of a vigorous mind aud the possession of a large amount of general information. We regret, therefore, that our duty coerces us to condemn where our desire would lead us to the adoption of a favourable opinion of a book which bears the stamp of severe and protracted labour. Let us advise Mr. Tod, in the first instance, to assume right premises, to divest his mind of all preconceived opinions, to avoid shadowy hypotheses, to treat of reasonable facts, and to give up his well-marked predilections for quackery; and we have no doubt that when we again have to notice a work emanating from him, our verdict will be more than favourable, and creditable to the science which it may conduce to advance.

Mémoire sur les Luxations des Cartilages Costaux. Par LOUIS SAUREL, D. M. M., &c. Montpellier: Patras. 1854. 8vo. pp. 46.

AMONG the injuries to which the body is liable, some excite our attention from their extreme gravity, as fractures of the cranium or pelvic bones; others, as dislocation of the humerus, from their comparative frequency and the necessity of prompt treatment; and as this must depend on the utmost accuracy of diagnosis, we are careful to study all the symptoms of the accident, as well as those of all the other injuries of the joint, that might be mistaken for it. Some few again are recommended to our notice by their rarity; of this class is the dislocation which has engaged the attention of M. Saurel in the memoir before us. Luxation of the costal cartilages is seldom met with, and when it is, the injury of internal organs, the lungs, liver, or heart, which result at the same time from the extreme violence which has caused it, often entirely mask the less important lesion. Few cases had been previously related by authors; Garengeot, Martin of Bourdeaux, Boyer, and M. Leger, have given observations more or less perfect of dislocation of the costal cartilages known to themselves. Some difference of opinion has existed as to whether the displacement of the costal cartilage from the rib is not rather a fracture than

a true dislocation. The author, however, relates three cases where it was supposed to exist, by MM. Bouisson, De Kempe, Benoit, and two by himself; at the same time he expresses his belief, that they were more likely to be fractures or solutions of continuity in the cartilage itself, than displacement of the end of the cartilage from its articulating cavity. He was confirmed in this opinion by experiments which he made on the dead subject. From these experiments, and careful deductions from the cases which he relates at length, M. Saurel gives the following *resumé* of the examples of the accident:—

"The deduction may fairly be drawn from the facts which I have adduced, that considerable violence is necessary to separate the ribs from their cartilages; that this separation is more like a rupture than a dislocation; that a true fracture may exist without our being able to distinguish it, during life, from what is called dislocation: finally, that even in cases where neither mobility nor displacement are observable, a rupture of the costal arch may exist in the exact spot where the cartilage is united to the rib.

"Whatever may be the true nature of this lesion, to which we attach the name of chondro-costal dislocation, it is important to study the causes which give rise to it, the symptoms by which we can recognise it, and the means of treatment best adapted for it.

"With respect to age and sex we have nothing to say. The subjects of the former observations that we have related had reached adult age; the one under my care was even above 50. It may, however, be believed, that youth and adult age are more favourable to the production of this displacement than old age, for in the last, fracture is easier than dislocation.

"In the cases of MM. Bouisson and De Kempe, we see that the displacement was situated on the fourth and fifth ribs; in the two others, on the contrary, the ninth rib was its seat.

"It was not always produced by the same cause; direct external violence seems oftenest to have given rise to it; this was seen in the first and second cases. Sometimes it was produced by *contre-coup*, when strong pressure was exerted over the free end of the cartilages, as in the third case. Finally, sometimes, as the fourth case proves, muscular action may be powerful enough to separate violently the bony rib from its cartilage. The comparison of the four facts already related seems to prove, that the dislocation of the sternal ribs would take place most frequently by direct violence; while indirect violence and muscular contraction would more readily produce displacement of the asternal ribs. The want of flesh,

and an energetic dilatation of the thorax at the instant of the accident, are, setting aside other organic or constitutional predispositions with which we have no reason to occupy ourselves, circumstances which appear favourable to the displacement in question."

The symptoms which we find specified in the observations as yet known are the following: violent pain, sometimes followed by insensibility, and accompanied, in certain cases, by a feeling of something having given way; a sense of oppression, painful respiration, which increases the suffering at the seat of injury; externally, more or less ecchymosis, so much so as to mask the injury; more or less swelling according to the time we are called in. In examining the seat of the lesion, we recognise the existence of a more or less marked depression at the costal or cartilaginous extremity. According to Sir A. Cooper, it is the cartilage which makes the external projection; whereas in the observations of MM. Bouisson, De Kempe, and Benoit, this projection was manifestly formed by the costal extremity; the depression corresponded, therefore, to the cartilage. In the case observed by the author, this important sign was wanting. When one felt the extremity of the rib no inequality was found to exist, and the separation was even. Inspiratory movements have generally the effect of making the cartilage ascend to its place, and reduce the luxation, while it is reproduced during expiration. Pressure exercised with the fingers on the dislocated end can, in the absence of other signs that we have just indicated, cause the dislocation to be ascertained. During the displacement thus produced, a particular noise is heard which the patient himself is sensible of. These manœuvres are always painful.

"These are the principal characteristics by which we recognise the chondro-costal dislocation; in many cases they will be sufficient to arrive at an exact diagnosis; in others, on the contrary, they will only furnish a sufficient degree of certitude. The other lesions with which we may confound it are fractures of the cartilages and ribs. Without doubt, in thin subjects, and in the sternal ribs, the projection forward by the chondro-costal articulation is sufficient to avoid any mistake; but in very fat subjects, where the dislocation is seated in the false ribs, and in the case where a considerable swelling has taken place immediately after the accident, it may be very difficult, if not impossible, to recognise the exact lesion we have to deal with. If the fracture of the cartilage is seated in a point very near the bony rib, how are we to distinguish this lesion from true dislocation? The displacement will be the same, and it

will be accompanied by exactly the same symptoms. The difficulty will not be less great if the costal extremity is broken in its thickness, as is shown in one of our experiments; in this case, moreover, all the symptoms will be those of a dislocation; especially if the chondro-costal periosteum is not broken, because then, the mobility being feeble, no crepitation will be produced. The error of diagnosis, however nearly inevitable in such cases, will be of no importance, since the treatment of these different lesions is always the same. The prognosis of the chondro-costal dislocation is not serious; this lesion is only so in cases when the wounding cause has borne its action on the organs contained in the thoracic cavity, or when the displacement affects many ribs at once. In such cases the injury may be mortal."

"With respect to the treatment called for by a chondro-costal dislocation, it is very simple. If the contusion is severe, if there is much difficulty in breathing, if there are much swelling and ecchymosis, we should have recourse to bleeding, general and local, and to soothing applications. The reduction of the displacement ought to be effected at once; it is generally easy. During inspiration the costal cartilage takes its proper place; we must profit by this circumstance to fix it there, and to oppose its consecutive displacement by means of a body bandage applied round the chest. If the reduction of the displacement does not take place naturally, it may be accomplished by pressure exercised on the end of the rib, and making the patient draw deep inspirations. If the cartilage has much tendency to displacement we ought, at the same time that we render the chest immovable, apply compresses graduated over the costal extremity, in order to oppose its projection. Finally, if the reduction is found impossible, it is better to leave the case alone, than by any operation to raise the depressed cartilage. Consolidation ought always to be effected by bony callus, exactly as in fractures of the ribs and cartilages."

On the Topical Medication of the Larynx in certain Diseases of the Respiratory and Vocal Organs. By EBEN. WATSON, A. M., M. D., Fellow of the Faculty of Physicians and Surgeons of Glasgow, &c. London: Churchill. 1854. 8vo. pp. 183.

WE have read this small volume with no little pleasure. Unlike many monographs, which we have from time to time had occasion to criticise, it seems to us to be peculiarly devoid of

that eyesore to medical reviewers,—irrelevant matter. Dr. Watson's object, as he states in his preface, is to explain the *rationale*, and to recommend the practice, of the topical medication of the larynx in certain diseases. To these two points he has scrupulously restricted his observations, and he has done so with a clearness and an earnestness which cannot fail to impress the reader with a most favourable idea of his merits as a scientific practitioner. In the light of a first appearance before the public, as an author of a regular treatise, we feel bound to say, that, without any peculiar claims to originality or perfection, he has produced a contribution whose many excellencies augur well for the success of future efforts. It is true that a large portion of the material has already appeared from his pen in the current medical literature of the day[a], but, so far from this being an objection, we believe that it presents a substantial reason for a higher appreciation of the work, inasmuch as the original observations have been, to use our author's words, "augmented by the results of a larger experience, and corrected by the suggestions of a more mature reflection."

It might be supposed that this work would savour much of *hobby* writing, from one or two observations which our author lets drop in his preface. Dr. Watson states he was one of the first, if not actually *the* first, in his part of the country, to practise the topical medication of the larynx. Now assertions of this nature are not necessarily called for, even by discoverers, and are apt to suggest the reflection that the writer is, to say the least, vain, or inclined to give undue credit to the value of his work. In the present instance, however, the reader will soon find that such a conclusion would be premature. So far from insisting upon the peculiar merit of the treatment recommended, after the manner of a zealot, our author observes:—

"I am far from wishing *the too ready* adoption of new procedures by the members of our profession, and I do not think this the inclination of the present time; but when, in a set of cases in which the ordinary treatment often fails, a new plan is recommended, as founded on known facts of clinical observation and of pathological theory, *then*, I do maintain it is *at least worthy of a trial;* and when the recommendation can be still further enforced by practical example, it would *almost seem* that it *ought* to be adopted, or that reasons against it ought to be produced."

This remark is rationally modest, and shows that our author is not carried away by any new idea. In another part he says:—

[a] See the November Number of this Journal, for 1850; and the pages of the Lancet for the year 1851.

"Such happy results are by no means *always* to be looked for, and I am *far from wishing* to laud the topical applications beyond what they deserve." [The Italics are our own.]

And, in further corroboration of our view of the spirit which pervades the work, we may here add, that he does not hesitate to mention unfavourable as well as favourable cases, and that he takes the utmost care to restrict the adaptability of the system enjoined to such species of cases as physiologically meet the requirements of the most guarded ratiocination.

So much for the general scope and character of the work before us. It now becomes our duty to put our readers in possession of such details in the management of his theme as individualize the author, and will impart a clear conception of the practical points which distinguish the volume.

The work is divided into ten chapters: the two first relate to the use of topical agents; the third, fourth, and fifth, illustrate the utility of the treatment in laryngeal disease; the sixth to the ninth, inclusive, refer to the benefit of the treatment in other disorders in which the laryngeal *nerves* are involved; and the last chapter is devoted to the laryngeal complications of pulmonary phthisis.

The illustrative cases, given somewhat in detail, are forty-four in number, and occupy fully one quarter of the entire work. Besides these there are numerous casual references to some which have occurred in his own and others' practice. It will thus be observed, that a large proportion of space is devoted to illustration,—we think, however, not too much, as the importance and novelty of the subject demand such a mode of proceeding. We could have wished, indeed, that the cases narrated had been pressed into narrower compass, by being set up in a smaller type, as this would facilitate the reader's comprehension while perusing the volume.

Dr. Watson commences his treatise by a reference to the writings of Sir Charles Bell, who, we are told, was *the first* to put on record a case illustrating the use of topical applications to the interior of the larynx. It appears he employed a forty-grain solution of the *nitras argenti*, and describes in his "Surgical Observations," published in 1818, his mode of procedure, and the circumstances under which he directed this new treatment. Our author refers in the same place, to a work of MM. Trousseau and Belloc, published in 1837. They were the first to use the whalebone rod and sponge attached. Three years after (and it would appear *independently*), Dr. Horace Green, of New York, formally introduced the new treatment before

the Medical and Surgical Society of that city; and, indeed, it is from *his* labours and devotion in this particular line of practice, that we may date the origin of all that has been accomplished of a satisfactory and practical character. Dr. Green has so identified himself with this particular manipulation, that he has made it quite a specialty in practice.

After passing some well-merited strictures upon the extraordinary pretensions of Dr. Green (amongst which we may mention, *en passant*, the assertion that he has pushed the probang not only to the division of the trachea, but, turning it aside, has passed it at will, in many instances, *into the right or left bronchus*, with as much ease and safety as the catheter is introduced into the bladder), Dr. Watson proceeds to describe laryngeal probangs, and the manner of using them. He places in a very clear light the precise extent of the respiratory membrane which it is possible to reach, and proves, by reference to the measurements of the length and distensibility of the aperture of the glottis, that, even in infancy and childhood, a proper instrument may be passed through the rima. As to the mode of using the instrument, the following paragraph is much to the point:—

"But it may here be objected, that although it be granted that the rima is sufficiently long and distensible to admit the probang in the dead subject, we cannot expect the same conditions to present themselves in the living. The slightest touch of the probang may well be supposed to cause complete closure of the glottis; and this does seem an almost insuperable obstacle, at first sight, to the operator. But he must begin his manipulations by educating the upper part of the larynx and the fauces themselves to bear the presence of the instrument before he proceeds further; and, when this has been accomplished, he must not think of forcing through the closed glottis. He must rather surprise it during an act of inspiration, when its muscles are relaxed and its aperture open, for then he will find the introduction of the sponge both safe and easy."

A little further on, a hint is given to the operator which he may well remember. Should the probang be felt as if detained on withdrawing it, he may be assured that the detention is produced by the contraction of the *pharyngeal* muscles, and that the sponge has not passed into the larynx at all. Dr. Watson here challenges a statement of Mr. Erichsen, made so lately as 1853, that the sponge has *never* been passed, in the living subject, beyond the true vocal chords; and maintains, that topical applications may be made throughout the whole length of the larynx, and also of the trachea. Here we have an illustration of the hackneyed adage, "doctors differ." It is a point, it must

be confessed, about which much might be written, but to decide which volumes would fail. There can be no question about the practicability of passing the instrument into the trachea *in the dead subject;* and instances are numerous where foreign bodies of considerable size have been drawn by an inspiration as low as the tracheal division. Still these facts do not give the required solution. Nothing short of the execution of the operative procedure, by means of a probang with a graduated stem, in the presence of a number of competent witnesses, would suffice.

Though we admit the correctness of Dr. Watson's view, judging from what we ourselves have observed, yet we must do Mr. Erichsen the justice of exercising a praiseworthy caution when he finds that his own observations have failed to convince him; and while it may be that he has not employed *all* the opportunities accessible to him for coming at the truth, we think that Dr. Watson has exhibited but little of his usual good taste when he brands his opponent with incompetency or prejudice.

The substances employed as topical agents by Dr. Watson are the nitrate of silver and the hyposulphite of soda and silver. Of the latter he has had but little experience, as it appears only adapted to a limited number of cases. The idea of using this new salt occurred to him after its recommendation by M. Delioux in some cases of urethral discharges. The manner of making this preparation is given according to M. Delioux's directions. "Thus: the oxide is precipitated from a solution of the nitrate of silver by potass, and re-dissolved in a solution of the hyposulphite of soda. On evaporation (which ought to be conducted in the dark), minute crystals of the hyposulphite of soda and silver are formed. They are very soluble in water, but not in alcohol: they may, therefore, be washed in the latter, and thus obtained perfectly free from impurity. The stars of these acicular crystals, deposited on a slip of glass, form a very beautiful microscopic object, especially when viewed by polarized light."

Dr. Watson considers that, if applicable in any case, it is so in those of vitiated secretion from the surface of a mucous membrane, and especially in those instances of pharyngo-laryngeal disease in which a strong stimulant would be hurtful.

The nitrate of silver is the salt which he has, however, chiefly used, as the one which has longest stood the test of time. In making solutions of it the *pure crystal* is always to be preferred; for, says Dr. Watson, if the solution contained impurities, they would adhere to the mucous membrane, and produce a

constant tickling cough till expelled, and might, in certain cases, excite or aggravate ulceration of the membrane.

Four solutions are employed:—No. 1, containing 10 grains to the ounce; No. 2, 20 grains; No. 3, 40 grains; and No. 4, 60 grains.

The *modus operandi* of this agent is illustrated by its action in the case of the excitement of inflammation in the web of a frog's foot. Thus, in the part which is most intensely inflamed (as in the inner circle whose centre is in a state of sphacelus), the solution, in the direct ratio of its strength, increases the stasis of the blood It acts through the coats upon the contained blood by causing its partial coagulation, and likewise by withdrawing water from the serum for the crystals about to form. In the outer circle, however, the stimulant solution causes a renewed and increased dilatation of the vessels, and the retarded current moves on in them more freely than before; a cure being thus speedily effected if the exciting cause of the inflammation has ceased to act. Hence (in the first place), a reason for its beneficial action in all varieties of the inflammatory process, except the most intense.

Secondly, inasmuch as a strong solution tends to remove the watery part of the blood in the vessels by the law of *exosmose* and *endomose*, its value in cases of œdema of the glottis may be deemed an inference. Thirdly, the excitement or irritability which characterizes the state of the *nerves* in a part inflamed may be relieved by the action of the solution, which, as a stimulant, will remove the cause of the irritation.

Such are the principles upon which the author bases his views of the special use of the nitrate of silver as a local agent in laryngeal disease.

Dr. Watson seems not to have given due weight, however, to the modifications induced by the interposition of mucus in laying down his idea of the action of the remedy. It is well known that the coating, formed by the nitrate in combination with animal matter, protects the tissue in a great degree from the action of the caustic. Instances are not infrequent in which the solid nitrate has been in large quantity *innocuously* applied to mucous surfaces. Nevertheless, we know, from experience of its topical use in external inflammations, that the morbid action is subdued in most cases. In ophthalmic disease this has been long manifest, and in erysipelas, and all forms of ulceration. Judging from analogy, therefore, we should expect beneficial results in inflamed states of the larynx: and, to Dr. Watson's explanation, we can make little objection, more especially when he advises the employment of that amount of sti-

mulation only which is suitable for the degree of inflammatory action present in each particular case. We shall see, however, in the rules he gives for its application in special diseases, whether that explanation will practically suffice to meet their requirements.

In the management of "simple acute laryngitis" Dr. Watson premises antiphlogistic measures, especially depletion and emetics. "It is," says he, "*after* the use of both these remedies that the topical application is alone admissible;" and further, in some severe cases, where there is danger of chronic thickening of the mucous membrane, he advises the use of mercury at the termination of the acute stage. During the action of this mineral the topical application of the nitrate should be stopped, to be resumed only after the mercury has had its required effect, when the local treatment is to be continued and repeated daily, until all symptoms of laryngitis have completely disappeared.

In "croup," however, as being essentially a very different disease, he enjoins a mode of management in some respects the opposite. In this disease he admits, indeed, that he has always found the symptoms of congestion *increased* by the application of even a weak solution of the nitrate. This, his experience, he observes, is apparently opposed to the results of Dr. Green's cases. We say *apparently*, for it appears that his success was not solely, if at all due to the use of the nitrate; and, moreover, it seems more than probable that, in the successful cases, the *early* treatment of the disease, prior to exudation, was the main point for which credit ought to be claimed.

In this latter stage, which Dr. Watson has termed the "pre-exudation" (which is in reality a catarrhal inflammation), the application of a fifteen or twenty-grain solution he has found of essential benefit; and in an ulterior stage, when, as not unfrequently occurs, sudden œdema of the submucous tissue happens, the utility of the topical remedy is fully as marked as in the original simple form.

The importance of a differential diagnosis between the œdema glottidis and croup is fully insisted upon; and justly so, as in many cases it is obscure. The *absence* of the vibration of the glottis during respiration, coughing, or speaking, is noticed as a chief distinguishing mark of œdema. This physical sign, or its absence, is of course to be ascertained by auscultation of the larynx, which we must admit has been ever too much neglected by the profession. We are glad to find, then, that Dr. Watson endeavours to fix attention on its value, and we believe his work will have this effect; if so, we can

readily foresee the vast improvement which will ensue in the management of laryngeal disease.

Among the *chronic* affections of the larynx, which Dr. Watson notices in his fourth chapter, he refers to and gives an instance of Dr. Green's "follicular disease," which seems to be an affection (if we are to believe the latter author) very common in America; but, from the evidence of recent pathologists there is reason to think this frequency more apparent than real, as all the laryngeal symptoms in Dr. Green's cases may be accounted for by other than follicular lesion, a lesion confessedly rare as a *post-mortem* appearance.

The combination of laryngeal and pulmonary disease is next alluded to in a practical and forcible manner. We all know how difficult it is sometimes to ascertain the exact degree and extent of pulmonary disorder when the larynx is engaged. The latter masks the former. Dr. Watson, under these circumstances, appeals to the auscultation of the larynx for a solution, and gives some important pertinent hints, and directs attention to the admirable monograph of Dr. Stokes on Diseases of the Chest.

Minute directions are here given as to the use of the nitrate solution in "chronic laryngitis." The frequency of the application he makes to depend, in these cases, upon the degree of local irritation induced, during the existence of which no renewal should be attempted. They require rather a *continued* application of solutions of a moderate strength for a considerable period, and the combined use of general measures adapted to the constitutional state, or any existing complication.

Several cases of *aphonia* are detailed in Chapter v. Amongst these is noticed the interesting observation of paralysis of the glottidean muscles. Dr. Watson is inclined to think that there is an inflammatory stage *prior* to paralysis, and also to the state of fatty degeneration and atrophy, to which the muscular structure under these circumstances is so liable. However this may be, in some cases it is manifestly *not* a prominent cause; but, as he has found the benefit of a local stimulation of the glottis equally beneficial in *these* cases, and in the very opposite, viz., undue excitement of the laryngeal nerves,—it seemed necessary that something like the pathology given should be established.

Case xx., indeed, is detailed with a view to show that, though there was no satisfactory proof of any well-marked inflammatory action having preceded the palsy of the glottis, yet complete relief was afforded by the local treatment. The difficulty is ingeniously encountered by a kind of pathological explanation, to the effect that over-exertion of the organ, when

too long continued, produces, after a degree of congestion, obstructed circulation and subsequent anemia, and even atrophy, —this obstruction being due to the tonic contraction induced by the continuance of the muscular effort; in fact, the impression of great fatigue. Dr. Watson admits that this species of explanation may be deemed rather strained; and when we consider that stimulation of the glottidean nerves is the desideratum in many cases of aphonia, there seems no necessity to have recourse to any such hair-splitting reasoning.

The author gives an excellent illustration of the manner in which relaxation, thickening, and ulceration of the mucous membrane of the tracheo-laryngeal tract may produce alterations in the modulation and tension of the vocal sounds. The explanation is based upon physiological principles established by Mr. Wheatstone and M. Savart; and it presents a very good idea of how the practice of the practitioner is made to hinge upon scientific data. The use, in such cases, of the caustic solution depends upon its stimulant effect on the relaxed vascular condition of the membrane. The thickening yields, in like manner, by improving the tone of the capillaries, and the ulceration heals by means of a similar influence being effected upon the circulation in the immediate *vicinity* of the ulcers. Dr. Watson concludes this part of his subject by an allusion to a very important, but not generally noticed, cause of aphonia, which we shall here quote as being at the same time a fair specimen of his style:—

"But another very fruitful source of aphonia, in some of its many degrees, is *speaking in an unnatural tone of voice.* Such a tone is apt to be assumed by a person of weak vocal power in his attempt to make himself heard in a large room; and it is also adopted, sometimes unconsciously, in imitation of one who has been frequently listened to. But it is needless for me to inquire into all the causes of the assumption of a false tone of voice. It is more to my purpose to point out its noxious effects on the speaker's voice, by its diminishing the ability of the vocal muscles to act for any length of time without excessive fatigue. Let any one attempt an unusual or unintended use of any other set of muscles, and he will soon be persuaded of the truth of this assertion; and surely the evidence brought forward in the preceding part of this chapter renders it unnecessary to prove that the muscles of the glottis form no exception to the general rule. It is, then, of great importance to every public speaker to ascertain the *natural* tone and pitch of his voice. Much may be safely done by management and practice to increase the power of the voice, if only it be used *within its natural range;* and if for no other purpose than the ascertaining of this range, every person who intends to speak much in public should begin by studying the tones

of his voice in singing as the most important lesson he can learn in elocution. I am quite persuaded that were clergymen to pursue some such plan as this in their early life, they would afterwards be less frequently affected with loss of voice; and I am supported in my opinion by the fact that barristers, who generally speak in a conversational tone, and professional actors and singers, who only educate the particular kind of voice with which they happen to be gifted, are very seldom the subjects of aphonia; yet no one will deny that they, especially the singers, exert their voices prodigiously, and far more than most clergymen require to do so, in the discharge of their duties."

The Chapter on " hooping-cough" is well worthy the attention of the profession. The remarkable results recorded of the use of the topical treatment in the hands of M. Jobert and the author clearly prove the value of this new mode of treatment. It appears from the statistics given at page 119, that 57·4 per cent. have been cured in two weeks; 36·5 per cent in three to four weeks; while only 5·3 per cent. resisted the treatment, and the minute fraction of barely 0·6 per cent. died. These proportions, we would say, are fairly given; for, though many of the cases which were longest under treatment had relapsed, the whole period from the commencement to the end of the treatment, without any distinction, in every instance, was taken into account. These results contrast very favourably, indeed, with the statements of the principal modern authors, the average of which would extend the duration of the disease from one and a half to three and a half months.

Dr. Watson, as usual, attempts the *rationale* of this mode of treatment; and in doing so we are inclined to think breaks new ground. His idea of the disease is simple. He considers the phenomena, characteristic and essential to the disease, to be the following: first, the poisoning of the blood; secondly, the inflammatory state of the pharyngo-laryngeal membrane; thirdly, the irritation or excitement of the laryngeal nerves, manifested by the characteristic *whoop*. He further believes, that the poison is generally eliminated, or rendered innocuous, by the time that the spasmodic stage has reached its acme, and that all inflammation, *beyond* what has been mentioned, should be deemed a complication. In this view of the pathology he has been partially borne out by Dr. Todd, who, however, considers *all* inflammatory action occurring in any case as complicating the disease.

As to the mode of action of the caustic solution M. Jobert's view is endorsed with but little variation, namely, that it operates mainly in subduing inflammation. In confirmation

of this view, Dr. Watson has ascertained, that the rapidity of cure by topical means is in an inverse ratio to the severity of the inflammatory action. The mere abatement of this last condition seems, however, to exercise but little effect in subduing the nervous excitement. This the experience of every practitioner manifestly proves; while it is under these very circumstances that the topical plan is most rapidly effective. Dr. Watson still lingers on the opinion, that the affection of the nerves in these cases is more or less of an *inflammatory* character. This point is difficult of proof, and, at best, it rests only upon assertion or analogy. Besides, if we admit that the nitrate can act upon the fibrillæ of the nerves as a sedative agent (as he himself, in a former part of the volume, seems to allow), there is no necessity for such an explanation.

An important objection to the topical treatment of hooping-cough is mentioned at page 114. Granted that this treatment should completely remove the local manifestations of the poison, it does not necessarily follow that the poison itself has become neutralized. This objection is met by the hypothesis, that the whoop, and, in short, all the symptoms ulterior to the catarrhal stage, are an evidence rather that a peculiar morbid poison *has been*, than that it *is then present* in the blood; and he adduces the instance of so-called *specifics*, whose action he declares is no very satisfactory proof that they are direct antidotes to the poisons which originated the diseases wherein they are specially employed. He then arrives at the conclusion, that the only reasonable plan of managing poison diseases is to shorten or cure the secondary effects. This is, at least, all that we have in our power, until we discover antidotes to their remote causes.

This matter, we cannot think, is destined to be settled by any process of reasoning. The touchstone of experience can alone decide to the satisfaction of the profession: and we are willing to give credit, so far as it goes, to that which Dr. Watson has furnished, which is thus embodied in the following sentence, viz.:—

> "If the disease be thoroughly subdued in its first attack, no recurrence of it need be apprehended during the patient's life. I make this statement in full knowledge of the succeeding history of all the cases in which I have employed the topical treatment; and many of them have been again freely exposed to infection."

The new treatment introduced by Dr. Arnoldi, and so strongly recommended by Dr. Gibb in an excellent monograph on hooping-cough, is briefly referred to in rather unfavourable terms. However open to objection Dr. Gibb's view of the

modus operandi of nitric acid in this disease may be, we feel assured that Dr. Watson has not done justice to its claims. His trial of the remedy has not been satisfactory either to himself, or, we will venture to add, to us; and we state so from some experience. We consider that Dr. Gibb deserves the thanks of the profession, for advocating so ably the use of this remedy, which, though it cannot lay claim to such rapid cures as the laryngeal treatment, has, in our hands, succeeded in cutting short the malady when compared with the ordinary routine treatment of our ancestors.

Dr. Watson draws attention, in the seventh chapter, to the state of the larynx in cases of "spasmodic asthma," a subject, which, he says, "has not hitherto received adequate attention, either from pathologists or physicians." He shows very clearly, we think, both from physiological reasoning and clinical observation, that spasm of the glottis is an *essential* part of a fit of asthma; and that, consequently, the topical treatment is pathologically indicated. We believe this is the true view. Hitherto the routine treatment of asthma has been rather an *opprobium medicinæ*. There was no certainty, no confidence. Remedy after remedy was tried, and too frequently with the same effect—failure. Dr. Watson very properly cautions his readers against cherishing the idea that all asthma cases will yield to the local treatment. It must be always understood that the cardiac form is an exception; and, we would add, all cases in which the substance of the lungs is engaged, or the contractility of the respiratory membrane permanently impaired.

The manner in which gastric disorder may sympathetically react upon the laryngeal nerves forms an interesting topic in the eighth Chapter. It is also distinctly shown that enlargement of the tonsils, and elongation of the uvula, may spring from disorder of the digestive system. So that, while we should never omit in the management of these cases the correction of the stomach affection, we cannot effect a complete recovery without such local treatment as will remove the inflammatory action of the pharyngo-laryngeal membrane, and abate the nervous irritability. This latter will be best realized by the free topical use of the nitrate. Even in "hysteric cough," somewhat of a similar plan may be judiciously adopted with good effect: for, however beneficial may be our remedies for improving the general health and the functions of the different organs in this protean malady, we must not forget, that local nervous irritation may *remain* to harass the patient. Similar observations are applicable to the management of "laryngismus,"

and "epilepsy" itself, of which the laryngeal spasm is, perhaps, the most important feature. Our author was, we believe, the first to recommend the topical use of the nitrate in this most obstinate disease. It is not advised, however, in the light of a curative agent; but rather one which will tend to ameliorate its severer symptoms, and give the patient the best chance for ultimate natural recovery by the wearing out of the disease. If, indeed, further experience should confirm Dr. Watson's view, it will be a glorious triumph for the cause of science.

The laryngeal complications of "pulmonary phthisis" constitute a set of cases in which much relief may be experienced from the harassing cough by topical treatment. Relief, we say, is all he claims, as effected by this plan; but all practitioners must admit that even this is a great step in the management of a disease which, under favourable circumstances, it is admitted, may lead to a spontaneous cure. We all know that it is not so much the existence of tubercular deposit, which produces the multiform and distressing symptoms that characterize a case of phthisis, as the associated *inflammatory* complications which so frequently arise in the course of the malady; and, when the larynx becomes the seat, it is manifest that much may be done by direct applications.

At page 168 Dr. Watson gives the following list of laryngeal complications in which he has employed the topical treatment:—

"1st. Incipient cases, in which cough was caused by—
- *a* Actual laryngitis.
- *b* Secondary nervous irritation of larynx.
- *c* Secondary irritation by bloody sputum.

"2nd. Advanced cases in which cough was caused by—
- *a* Laryngeal ulcers without a cavity in the lung.
- *b* Laryngeal ulcers with a cavity in the lung.
- *c* Laryngeal irritation by purulent sputum."

From this it will be seen that there is considerable scope for the use of the nitrate. In all, relief, partial or complete, may be obtained so far as the laryngeal condition is concerned; and, in the concluding words of our author, in whose sentiments we fully concur, "by alleviating or curing these morbid states of the larynx much distress may be saved to the patient, and time may be gained for the treatment of the pulmonary lesions and the constitutional disease."

Having thus analytically touched upon the chief points noticed in the volume before us, and having, to some extent, indicated the manner in which these have been treated, our readers will, we think, have no difficulty in agreeing with us

that the work is an able and original exposition of the value of the new treatment. In every line it appeals to scientific data and to philosophic judgment. The cases adduced seem to be given, not so much in illustration merely of its superior efficacy, as of the manner in which the local treatment *brings about* a recovery. Dr. Watson has fairly laid the whole matter before his brethren. He has stated the case rather as a judge would charge a jury than as an advocate pleading for a particular cause. He has shown wherein the treatment he recommends may fail, as well as where it may succeed. He has enunciated the circumstances which interfere with its action, and detailed the special indications for its adoption in each particular case. He does not say that this is our sheet-anchor in *any* case: but, upon the experience which he is enabled to furnish, and upon careful physiological and pathological reasoning, he asks his brethren to give the plan a trial, and thereby ascertain, on a scale which no single individual can command, how far it may be useful, and how far prejudicial. Not for the sake of science alone, not for the sake of supporting a pet theory, but on the broad ground of relieving suffering humanity, he calls upon the profession to decide for itself, whether the "topical medication of the larynx" is deserving or not of the commendation he has bestowed upon it.

As the work of a careful and clear-headed reasoner and accomplished practitioner, we strongly recommend it to the notice of the profession, fully satisfied that it is valuable, not merely for the direct information it contains, but also for the practical suggestions which will arrest the perusal of every page. Judging from this single specimen of his talents, we feel convinced that the author of this volume is destined to take a high place in the annals of medical literature.

Clinical and Critical Contributions to Obstetric Science and Practice. I.—*On Uterine Polypus: its Nature, Early Detection, and Treatment.* By ROBERT BARNES, M.D. (Lond.), Member of the Royal College of Physicians, &c., &c. London: Churchill. 1854. 12mo, pp. 44.

THIS is a very good resume of the knowledge we possess on the subject of uterine polypus, with a confirmation of the more recent views of certain pathologists by the author's own researches. Professor Vögel has shown that the structure of the so-called fibrous tumour of the uterus is microscopically identical with the proper tissue of the organ; and the author, following Dr. Oldham

and Dr. Crisp, has obtained the same results. Nay more, it would appear that the smaller cellular polypi partake of the character of the tissue from which they arise, so that two at least of the different kinds of polypi may be regarded as hypertrophy of existing structure, and not a new formation. The symptoms to which polypus gives rise are very well stated, but we think that the author would have done well to have entered more fully into the distinction between fibrous polypus and other growths of polypoid form, which, though not rightly included under the term, are practically very liable to be treated as polypi.

We quite agree with the author's remarks as to the diagnosis of intra-uterine polypus, but perhaps we are hardly disposed to attribute as much significance to the general symptoms; the truth is, we can never feel sure of the existence of a polypus until we either see or feel it, although we may have strong suspicions. For attaining this certainty in the case of intra-uterine polypi, we have no hesitation in concurring with Dr. Barnes in the propriety of dilating the os uteri by means of sponge tents. As to the treatment, provided the morbid structure is removed in a reasonable time, it does not much matter whether excision or ligature be employed. It is often of more consequence to attend carefully to the subsequent treatment. The danger is by no means necessarily past when the polypus is removed. Upon the subject of treatment the author's observations are so good, that a little more detail would be very acceptable.

On the Relative Merit of the two Operations for Stone: two Lectures delivered at the Royal College of Surgeons of England, May, 1854. By Frederick C. Skey, F.R.S. London: Churchill. 1854. 8vo, pp. 55.

In instituting any comparison, howsoever perfect, candour to admit the absolute merits of the things compared, an obvious desire to draw unbiassed conclusions, and logical accuracy in making the deductions, are essential to render it of a nature so as to be influential on the mind. This is true of all comparisons, and no less so in contrasting the different plans of treatment in the practice of medicine and surgery, particularly the various methods of operating, than in those arts and sciences of apparently greater magnitude, and whose importance is more widely estimated. To advocate the cause of any plan or principle is one thing; to criticise it, with a view to ascertain its exact merits,

and to establish its relative advantages, is another. In order to arrive at a just appreciation as to which is the better of any two given plans of treatment or methods of operating, the merits and demerits of each must be exposed and fairly weighed, the scale being turned in favour of the one or the other according as the former preponderate over the latter. It would be well if this were more generally remembered, for the decisions arrived at are rarely impartial. Prejudice has ever been the great barrier to the progress of improvement in every art and in every science. To it may be ascribed the opposition with which the treatment of aneurism by compression has been met, which has caused a blind adhesion to one particular method of treating stricture of the urethra, and which has divided the profession into parties advocating the most conflicting sentiments on various subjects. Thus it is we have pure lithotomists and pure lithotritists. It is not to be supposed that those who adopt this one-sided course are always insincere and wilfully blind. The mind becomes unconsciously prejudiced, and impressions first formed, or made under peculiar circumstances, take such possession of it as to envelope it in a cloud, and prevent the light of conviction from gaining access.

With respect to the question which it is the object of the present pamphlet to determine,—"the relative merit of the two operations for stone,"—we feel ourselves bound fully to concur in the author's sentiments, so strongly expressed in the Preface:—

> "If conservative surgery be a desideratum, if preservation, not only of a part, but of the whole machine, which is equivalent to life, be the object and the necessary issue of scientific inquiry into the best means of alleviating disease, we may class the operation of lithotrity among the most valuable resources in modern surgery."

Let us not, however, prejudge the point, but draw our conclusions after it has been investigated.

First,—What are the advantages of lithotrity? Its chief advantage consists in the fact of its not being a cutting operation, by which it is, compared with lithotomy, exempt from the dangers of inflammation. Erysipelas, different kinds of diffuse inflammation, sloughing of the areolar tissue of the pelvis, and peritonitis, are common consequences of lithotomy; whereas lithotrity, though not free from such dangers, is much less liable to any one of them. When these evils do arise as the result of lithotrity, they are much more likely to be attributable to want of skill or caution in the performance of the operation than is the case in lithotomy. Comparative exemp-

tion from danger, then, is the chief recommendation which lithotrity possesses, and which gives to it an obvious superiority over the sister operation. The advocates of lithotomy, however, can launch three of what they may view as powerful arguments against lithotrity, and exultingly exhibit the former operation in strong contrast with it: one is the tediousness of the cure; another is the chance of relapse from the non-expulsion of every particle of the calculus from the bladder; a third is the chance which exists of some of the fragments becoming impacted in the urethra. Let us weigh each of these arguments fairly. The tediousness of the process of cure is only a disadvantage, it is not an objection to lithotrity; and since its degree must depend on the size of the stone and its physical peculiarities, the argument on that ground cannot have universal applicability. A small, friable stone may be completely reduced by the lithotrite at one sitting. Again, if the length of time with which a calculus is to be got rid of comes to be a consideration in any given case, lithotrity is contra-indicated; the argument, therefore, against it, grounded upon the tediousness of the cure, has but little weight in the cases fit for the performance of that operation. The second argument,—the danger of relapse from an unexpelled fragment becoming the nucleus of another calculus, possesses, perhaps, less weight than the former, since such an event can rarely occur except from the want of proper attention on the part of the surgeon. In no instance should a patient be pronounced perfectly cured from his calculus by lithotrity until he has been sounded frequently, under different circumstances, and if the fragment should be so small as to escape detection, it must, in all probability, be small enough to pass through the urethra, and be expelled as the others. The third argument, the risk of fragments becoming impacted in the urethra, certainly possesses some force, but not to the extent that might at first appear, and which the opponents of lithotrity would wish to press. It is not often that fragments become arrested in the urethra; and when they do, it is rarely they give rise to any serious consequences. The fragment can be frequently pushed back into the bladder, or drawn out of the canal with a forceps or scoop, and where this cannot be accomplished, the fragment will often make its way forward, without any artificial assistance, after a short time. Out of six or seven cases of lithotrity which are in our recollection at present, in only one did impaction occur so as to be productive of any annoyance. In this case the bladder was irritable and the mucous lining of the urethra was in a state of congestion, and the calculus fractured into peculiar

angular fragments; complete retention of urine, however, never occurred, so that it did not become necessary at any time to cut down and open the canal.

Such are the principal arguments urged against lithotrity, and such its principal disadvantages; and we shall now array these against the disadvantages of lithotomy. "It would," observes Mr. Skey, "be useless to discuss the subject of the relative merits of the two operations, unless we have a clear starting point by which to gauge the question of merit or demerit."

The "consequences which either retard recovery, or which lead to a fatal result" in lithotomy are thus enumerated by the author:

"1. Collapse without loss of blood.
"2. Hemorrhage occurring immediately, or consecutively.
"3. A protracted operation, from one or various causes.
"4. Wound of the rectum.
"5. Inflammation of the bladder, involving the substance of the organ.
"6. Sloughing from infiltration of urine."

This is a formidable array of evil consequences, and when added to those we have already alluded to they make the operation of lithotomy one indeed of a most serious nature. With respect to the mortality of lithotomy, Mr. Skey arrives at the conclusion, taking the average "in persons of all ages," and taking the average of the reports of the operation as performed in France as "one in five;" "can it then," he says, "be reasonably asserted that positive danger to life does not attend on the operation of lithotomy?"

Instead, however, of entering into any elaborate argument as to the merits or demerits of each operation, the author has preferred simply to state the evils of lithotomy, and to examine more in detail the "injurious consequences" of lithotrity, leaving conclusions to be drawn by inference. The latter are—

"1. Protracted and occasionally severe pain.
"2. Inflammation of the mucous membrane of the bladder.
"3. Lodgment of fragments of stone in the urethra.
"4. Hemorrhage from the bladder or urethra.
"5. Extravasation and abscess from rupture of the mucous membrane of the urethra.
"6. Collapse from disease, aggravated by a series of operations of the urinary system, involving either a sacculated bladder, from the cysts of which the remaining fragments of the stone cannot be disengaged, or positive disease of the kidneys themselves.
"7. The supposed difficulty of removing every fragment from the bladder."

The third and last consequences, however, together with one of the disadvantages mentioned before, the tediousness of the process of cure, are those only which, strictly speaking, apply to lithotrity; all the others enumerated occur almost invariably as the result of the operation unskilfully performed or injudiciously undertaken. The possibility of anything being abused is no argument against its use; so the possibility of an operation being rendered dangerous by ignorance is no argument against its employment when it can be properly carried out.

With respect to the first consequence we agree, to a certain extent, with Mr. Skey's remarks—

"1. With regard to *physical pain*, there is no doubt that it is the attendant on both the operation and the after treatment. The question is not dissimilar from that of *danger* to life. We suffer a *multitude* of positive evils rather than encroach within the circle of *one* danger; and we gladly compound for a repetition of many smaller sufferings, to avert the real misery of a single large one. The *degree* of suffering from the action of the lithotrite may, however, be inferred from the fact that we seldom resort to the employment of anæsthetic agents to mask our operation; still, pain is an evil to be thrown into the scale against the operation."

As regards the fourth consequence, "hemorrhage from the bladder or urethra," he says—

"Certainly, it is an uncommon event, and when present, is only occasionally severe, and still more rarely is it as serious as in the case I have quoted. The question may be asked, does it in any material degree retard recovery? I think not; and if I am not incorrect in my creed, that without violence to the bladder, always to be restrained by the operator, it will rarely, if ever, occur, it will take its position among the most unusual events incidental to the practice of lithotrity."

In all the instances of lithotrity which have come across us we have never known hemorrhage to any amount to result from the operation. It is no uncommon thing for the urine to be tinged with blood for two or three days after the operation, and which seems, as Mr. Skey observes, to be unproductive of harm, but anything approaching to serious bleeding from the bladder we have never witnessed. Any one who has performed lithotrity half a dozen times, and knows the ease with which a calculus can often be caught and crushed in the bladder by means of the lithotrite, must feel convinced that where more than a very slight bleeding occurs, either the instrument must have been rudely and unskilfully handled, or the mucous coat of the bladder have been unsound. The fifth consequence of

lithotrity, "extravasation and abscess," is obviously attributable altogether to mismanagement or unjustifiable rudeness on the part of the operator.

"If from any cause the mucous membrane is torn and separated from its sub-tissue, the efforts at micturition, when unusually potent, force the urine from the channel into this tissue, and abscess is almost inevitable."

Further on he observes,—

"If in the perineum, that region will become swollen as in the early stage of ordinary perineal abscess; if more forward, a thickening may be felt along the track of the corpus spongiosum, varying in form and size. This swelling, when opposite the scrotum, occasionally presents itself forwards in the form of a conical thickening, the base of which is placed on the urethra, and is quite movable under the hand, so much so as to be readily mistaken for the testicle. If it attach itself to the lower part of the canal, in the neighbourhood of the membranous and prostatic parts of the urethra, and especially if posterior to the triangular ligament, its consequences may be most serious, because the escape of urine will probably be large, and the communication with the pelvic tissue more than merely probable. Moreover, the nature of the injury is in this region more obscure, and less tractable."

The sixth consequence, "collapse from disease," is also the fault of the surgeon. The author truly remarks that—

"An operation in this condition of the urinary organs is in contravention of sound judgment, and correct diagnosis."

Again, he very properly says:—

"If disease exists, it is our duty, if possible, to detect it by inquiry and examination, and to reject the case as inapposite to the operation."

The author then gives a couple of cases in illustration, and makes some very good practical remarks, particularly on "sacculated bladder." The last consequence of lithotrity we have already dwelt upon, and we have shown the amount of weight to be attached to it: we allude to the "difficulty of removing the last fragment of stone." Mr. Skey has shown his opinion as to the unimportance of this consequence, which, as before stated, has been used as an argument against lithotrity, when he designates it the "supposed difficulty." He observes—

"If it be so small as not to exceed the caliber of the canal, it will of course pass away without difficulty. If it be too large to reach its destination through the canal, I maintain that it requires

no erudite tact to detect it with a *fine sound*, well and carefully employed in exploring the bladder. This inquiry may be made, and repeated with instruments of varying form and magnitude. The best resources, both of touch and hearing, are of course brought into requisition in perfect silence, but beyond this no refinement is necessary. We have, indeed, no evidence in the reported cases of relapse, that re-formation of stone is attributable to this cause."

The author next enters upon the consideration of the circumstances which forbid lithotrity: "I shall," he says, "accomplish this end more readily by adopting the negative, and by stating under what symptoms the operation of lithotrity is *not* the operation to be selected." He then describes the steps of the operation itself, and, with respect to the most important one, the seizure of the calculus, he says:

"If, on having expanded the instrument, the lower blade be pressed downwards towards the rectum, by the elevation of the handle, the bladder will assume a conical form, the apex of which is directed downwards. Into the apex of this cone the stone will fall *three times out of four*, and I believe I may say in a yet greater proportion. I have myself caught the stone on one occasion ten times in succession, and I have repeatedly fixed the stone nine times, the blades being expanded and closed twelve. No action can be more simple, or more easy of execution. If the stone adhere to the coats of the organ, or if it fail, from any other cause, to fall into the concave blade, a slight shake of the instrument, or, what is less annoying to the patient, a slight shake given to the pelvis with the open hand, will generally succeed."

We are fully aware of the ease with which the calculus can be frequently caught by a lithotrite in the bladder when the manœuvre above described is adopted, still it is not so easy *always* as Mr. Skey would lead us to believe. In order to catch a stone quickly and with precision the manœuvre alluded to must be observed, but it will fail altogether in many instances from a variety of causes, and even in those in which it succeeds a variety of causes will influence the ease or difficulty with which it accomplishes its object. As to the practice of seizing a calculus as often as nine, ten, or twelve times in succession, it is, in our mind, to be condemned, no matter how easy of accomplishment it may be; six times in succession is frequent enough in any case, and, unless the bladder be extraordinarily placid, anything more than this it will seriously resent. So far as the accidents connected with lithotrity are concerned, they are—

"1. The convex blade of the lithotrite may be so strained by the pressure of the screw as to fail in its complete closure into the

corresponding blade. 2. The blades may be so clogged by fragments that it is impossible, by reason of their accumulation, to push home the convex blade. 3. The near or convex blade may snap off in the act of crushing the stone."

"Such are among the accidents to which the operation of lithotrity is liable; and if their importance is to be tested by their severity, let them also be judged by their infrequency. In the aggregate of cases they are rare; and the worst alternative that the most serious form can involve is an appeal to the knife, and to extract the stone by means of lithotomy. This refers to the fracture of the instrument, for which we have obviously no other resource."

Such is a very fair and candid exposure of the consequences, and accidents, and difficulties, and dangers of lithotrity, and we think that few, after carefully weighing them, and aware beforehand of the nature of lithotomy, will withhold consent from Mr. Skey's ultimate conclusion, viz.: "the certain presence of *danger* inseparable from one form, and its occasional though rare presence in the other."

As to the manner in which Mr. Skey has endeavoured to decide "the relative merit of the two operations for stone," we feel bound to state that he has accomplished his task with ability, and in perfect fairness to both sides of the question. He has also dealt with the subject as one practically acquainted with lithotomy and lithotrity; and his experience having been extensive, his observations must carry with them considerable weight.

Transactions of the Belfast Clinical and Pathological Society for the Session 1853–54. *With List of Members, Laws of the Society, and Report of the Council; to which is added, a Catalogue of the Pathological Museum.* Belfast: Mayne. 1854. 12mo, pp. 132.

We have rarely been more gratified at the receipt of a work than when the "Transactions of the Belfast Clinical and Pathological Society" was placed upon our editorial table. A proof of the energy, talents, and perseverance, of our professional brethren in the northern capital of Ireland we needed not: of this our own pages afforded abundant evidence for years past; but the result of their conjoined labours to advance medical science and literature, as exhibited in this well-got-out and carefully edited little volume, shows that their newly established Society may fairly take its place alongside the Pathological Societies of Dublin (the first established), and of Lon-

don. It is really highly creditable to the officers, and to every member who has furnished his contribution to so excellent a repository of rare and interesting cases. The beginning is, in this instance, more than half the battle, and from such a commencement we augur many valuable additions for years to come to the literature of our profession.

The Book of Prescriptions: containing 2900 *Prescriptions collected from the Practice of the most eminent Physicians and Surgeons, English and Foreign. Comprising also a Compendious History of the Materia Medica of all Countries, alphabetically arranged, and Lists of the Doses of all Officinal or established Preparations.* By HENRY BEASLEY. London: Churchill. 1854. Post 8vo, pp. 543.

The Practitioner's Pharmacopœia and Universal Formulary; containing 2000 *Classified Prescriptions, selected from the Practice of the most eminent British and Foreign Medical Authorities; with an Abstract of the Three British Pharmacopœias, and much other useful Information for the Practitioner and Student.* By J. FOOTE, M. R. C. S. L., &c. London: Renshaw. 1855. Post 8vo, pp. 368.

As an example of how a work undertaken may be very indifferently or extremely well executed, we have classed together the two books the titles of which are prefixed. Both prepared with good intent, to place in the hands of the junior practitioner a "ready reckoner" of prescriptions, one may be regarded as a useful and trustworthy assistant, the other as a careless and worse than indifferent compilation. Mr. Beasley has brought to his aid the skill and ability in selection which he had previously displayed in his "Pocket Formulary," and "Druggists' Receipt Book," the several editions of which have been noticed in our pages at the time of their publication; while Mr. Foote, exhibiting not alone carelessness, but reprehensible ignorance, has published a farrago of prescriptions—good, bad, and indifferent, old and new, chemical and unchemical—all strung together without rhyme or reason. The former, therefore, we warmly commend to our readers; the latter we altogether condemn.